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THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

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OCTOBER 1833....APRIL 1834.

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CONTENTS.

ART. I. Historical Eloge of ALEXANDER VOLTA. By M. ARAGO, Permanent Secretary to the Academy of Sciences of Paris, - - - Page 1

II. Instructions for Observers of the Aurora Borealis,—their Height, Sound, Periods, Effect on the Magnetic Needle, and Mode of Observation recommended, - - - - 35

III. On a Fossil Tooth found in a Red Sandstone above the Coal Formation in Berwickshire. In a Letter from Dr R. E. GRANT, - - 38

IV. The Rattle-Snake (Crotalus horridus, Linn.) disarmed by the Leaves of the White Ash (Fraxinus Americana, Mich.) Communicated by Judge WOODRUFF to Professor SILLIMAN, - - 43

V. A Sketch of the Tertiary Formation in the Province of Granada. By C. SILVERTOP, Retired Brigadier in the Service of H. C. M., K. of the R. and O. of Charles III., and F. G. S. With Plates. Communicated by the Author. (Concluded from Volume XV, p. 377), - - - 45

VI. On the Colour of the Atmosphere and Deep Water. By the Count XAVIER DE MAISTRE. (Concluded from former Volume, p. 359), - - 56

VII. Geology of the Valley of Oodipoor. By JAMES HARDIE, Esq. Bengal Medical Establishment, Member of the Asiatic Society, &c. Communicated by the Author. (Continued from Volume XIV, p. 282), 59

VIII. Remarks on the Gypsies, - - 67

IX. Electro-Magnetic Experiments. Communicated by the Author, - - - 71

ART. X. On the Origin of Meteoric Stones. By F. G. FISCHER, Esq. - - - Page 75
XI. On the Development of Heat in the Flowers of the Caladium pinnatifidum. By Dr E. H. SCHULTZ, of Berlin, - - - - 88
XII. Address delivered in the Senate-House at Cambridge, June 25, 1833, on the occasion of the Opening of the Third General Meeting of the British Association for the Advancement of Science. By the Rev. W. WHEWELL, M. A. Fellow and Tutor of Trinity College, and one of the Secretaries of the Association, - - - - 90
Astronomy, - - - 92
Light, - - - 95
Heat, - - - 97
Meteorology, - - - ib.
Geology, - - - 98
Mineralogy, - - - ib.
Physiology, - - - 100
The value of Theory in Science estimated, ib.
Morals of Science, - - 104
XIII. Projected Expedition into Central Africa, - 107
XIV. Formulæ for Trigonometrical Surveying. By Mr WILLIAM GALBRAITH, A. M. Communicated by the Author, - - - 110
XV. On the State of Medicine in European and Asiatic Turkey. By Dr OPPENHEIM. (Concluded from p. 273 of Volume XV.) - - 114
The Harem, - - - ib.
Turkish Cosmetics, - - 118
Hanging a Company of Chemists, - ib.
Early Maturity of Women, - - 119
Turkish Population decreasing, - ib.
Scarlet Fever and other Diseases, - 120
Uses of Bezoar Stones, &c. - 123
Ague, - - - 124
Gout not common in Turkey, - ib.
Erysipelas in Turkey, - - ib
Dogs do not become Mad in Turkey, 125
Standard of Morality among the Turks, ib.
Eaters of Opium and of Corrosive Sublimate, 126

Insanity and Suicide in Turkey, - 127
Idiocy produced artificially in Children and Adults, - - - 129
Practice of the present Sultan, - ib.
Lunatic Asylums the only Hospitals in Turkey, 130
State of Surgery in Turkey, - ib.
Observations on Eunuchs, - 135
The Dumb great Favourites, - 136

XVI. Observations on the Structure of Recent and Fossil Coniferæ. By William Nicol, Esq. Lecturer on Natural Philosophy, &c. Communicated by the Author. With three Plates, - - 137

XVII. Establishment of Captain R. Wauchope's Signal for ascertaining the Rates of Chronometers at the Royal Observatory, Greenwich, - - 158

XVIII. Memoir on the Question,—whether any Land Animals have ceased to exist since Man's Formation; and whether Man has been contemporaneous with species now lost, or appearing no longer to have representatives on the Earth? By M. Marcel de Serres, - - - - - 168

XIX. Description of several New or Rare Plants which have lately Flowered in the neighbourhood of Edinburgh, but chiefly in the Royal Botanic Garden. By Dr Graham, Professor of Botany in the University of Edinburgh, - - - - 175

XX. Proceedings of the Royal Society of Edinburgh, 181

1. Observations on the Anatomy of the Rorqual (a Whalebone Whale of the largest magnitude), drawn up from the dissection of a specimen found dead off North Berwick. By Robert Knox, M.D., F.R.S. Ed. - - - ib.
2. Continuation of "Experimental Researches regarding certain Vibrations which take place between Metallic Masses having different Temperatures." By James D. Forbes, Esq. Professor of Natural Philosophy in the University of Edinburgh, - - - - 183
3. On the Equations of Loci traced upon the surface of the Sphere, as expressed by spherical coordinates. By T. S. Davies, Esq. F.R.S.E. 184

4. Experiments and Observations on the Arterialization of the Blood. By WILLIAM GREGORY, M. D., F. R. S. E., and W. J. IRVINE, Esq. 185
5. Observations on the Lines of the Solar Spectrum, and on those produced by the Earth's Atmosphere, and by the Action of Nitrous Acid Gas. By Sir DAVID BREWSTER, LL. D., F. R. S. 186
6. Notice relative to the Pigmentum Nigrum of the Eye. By THOMAS WHARTON JONES, Esq. 189
7. On the Composition of some Iron Slags. By J. F. W. JOHNSTON, Esq. F. R. S. E. - 190
8. Communication relative to the Fresh-Water Limestone of Burdiehouse, near Edinburgh, belonging to the Carboniferous group of Rocks. By Dr HIBBERT, - - - - ib.
9. Additional Particulars relative to the Saurian Remains found in the Burdiehouse Limestone. Communicated in a Letter to Professor JAMESON. By Dr HIBBERT, - - - 192
XXI. Proceedings of the Wernerian Natural History Society, - - - - - 195
XXII. Proceedings of the Society of Arts for Scotland, 197
XXIII. Statutes of the University of Edinburgh, relative to the Degree of M. D.—1833, - - 200
XXIV. List of Patents granted at Edinburgh, from 24th June to 27th August 1833, - - 204

CONTENTS.

Page

ART. I. On the Thermometrical State of the Terrestrial Globe. By M. ARAGO, - - - - 205

I. At the origin of all things the Earth was probably incandescent; and even now contains a large portion of its primitive heat, - - 206

II. The Earth, then, was at one time fluid, - ib

III. Is there any means of determining for how many centuries the Earth has been cooling down? - 208

IV. In two thousand years, the general temperature of the mass of the Earth has not varied the tenth part of a degree. The demonstration of this proposition is derived from the Orbit of the Moon, - ib.

V. Does the original heat of the globe, still so apparent at a certain depth, contribute, in a marked manner, to the actual temperature of the surface? 216

VI. Is the Temperature of celestial space variable? And can this temperature become the cause of changes in terrestrial climates? - - 217

VII. Can the variations which certain astronomical elements undergo, sensibly modify terrestrial climates? 219

VIII. Of Terrestrial Climates, as they may be known from Observations made in different Ages, - 222

IX. The Mean Temperature of Palestine does not appear to have changed since the time of Moses, 224

X. Of the Climate of Europe in Ancient Times, 227

XI. Certain parts of Europe were not colder formerly than they are now, - - - 233

XII. Certain parts of Europe were not formerly hotter than they are at present, - - ib.

XIII. Of the Climate in the neighbourhood of Rome, ib.

XIV. Change of the Climate of Tuscany, - 235

XV. On Changes in the Climate of France, - 237

XVI. Conjectural causes of the increasing Coldness of the Summers of France and England, - 240

ART. II. Some Experiments and Observations on the Combinations of Carbonic Acid and Ammonia. By JOHN DAVY, M. D. F. R. S. Communicated by the Author, - - - - - 245
- 1. On the direct Combinations of Carbonic Acid and Water, - - - - 246
- 2. On the Sesquicarbonate of Ammonia, - 248
- 3. On the Bicarbonate of Ammonia, - 254
- 4. On the Effect of Heat on the solid Sesquicarbonate and Carbonate of Ammonia, - - 257

III. Statistical Views of the Mortality in various Countries in Europe, - - - - 259

IV. On Electricity, and on the Natural Sources of Electricity and Magnetism. By Professor M. A. DE LA RIVE, - - - - 266
- I. Atmospheric and Animal Electricity, - ib.
- II. On Terrestrial Magnetism, as connected with the Origin and Formation of the Earth, - 268

V. Geology of the Valley of Oodipoor. By JAMES HARDIE, Esq. Bengal Medical Establishment, Member of the Asiatic Society, &c. Communicated by the Author. (Concluded from Vol. XV. p. 59.) 278

VI. On the Animals depicted on Antique Monuments. By M. MARCEL DE SERRES. (Concluded from p. 175.), - - - - 285

VII. Observations during a Voyage from England to Fort Vancouver, on the North-west Coast of America. By Dr MEREDITH GAIRDNER. In a Letter to Professor JAMESON, - - - 290

VIII. Proofs that the Human Bones and Works of Art found in Caves in the South of France are more recent than the Antediluvian Bones in these Caves, 302

IX. Additional Observations on the Structure of Recent and Fossil Coniferæ. By WILLIAM NICOL, Esq. Lecturer on Natural Philosophy. With a Plate. Communicated by the Author, - - 310

X. Characters of Three Genera of Indian Plants. By G. A. WALKER ARNOTT, Esq. A. M. F. L. S. &c. 314

ART. XI. On the Structure and Habits of the *Limnoria terebrans*, a minute Crustaceous Animal, destructive to Marine Wooden Erections, as Piers, &c. By JOHN COLDSTREAM, M. D. M. W. S. Communicated by the Author, - - 316
History, - - - - ib.
I. External Characters, - - 318
II. Anatomical Details, - - - 319
III. Performance of Functions and Habits, 325
IV. History of the Ravages committed by the Limnoria, - - - - 328
V. On the purposes which the Limnoria is fitted to serve in the economy of Nature, - 332

XII. Inquiries respecting the Weight of Man, at different Ages. By M. QUETELET, - 334

XIII. Cuvier as a Naturalist. By C. L. LAURILLARD, Conservator of the Cabinet of Anatomy in the Museum of Natural History of Paris, - 340

XIV. Some Remarks on the Plant which yields the Cascarilla Bark. By DAVID DON, Esq. Libr. L. S. &c. Communicated by the Author, - - 367

XV. Remarks on Mr NICOL'S Observations on the Structure of Recent and Fossil Coniferæ. By WILLIAM MACGILLIVRAY, A. M. F. R. S. E. &c. 369

XVI. On a Method of so far increasing the divergency of the two Rays in Calcareous Spar, that only One Image may be seen at a time, - - 372

XVII. On Petroleum or Mineral Oil. By Dr REICHENBACH, - - - - - 376

XVIII. On the Berlin Cast-Iron Ornaments, - 384

XIX. Additional Notices relative to the Fresh-water Limestones in the vicinity of Edinburgh, belonging to the Carboniferous group of Rocks. By Dr HIBBERT, - - - - - 386

XX. Meteorological Table, extracted from the Register kept at Kinfauns Castle, North Britain. Lat. 56° 53′ 30″. Above the level of the Sea 150 feet. By the Right Hon. Lord GRAY, - - 389

ART. XXI. SCIENTIFIC INTELLIGENCE, - - 390

ZOOLOGY.

1. Low, the Orkney Naturalist, - - - ib.
2. Wood's New Work on the Mammalia, - - 391
3. On Migratory Habits of certain Species of Hirundo and Sylvia, - - - - - - ib.
4. On the Habits of some Land Shells, - - 392
5. Shower of Fishes, - - - - - 393

ART. XXII. NEW PUBLICATIONS.

1. Illustrations of the Botany and other Branches of the Natural History of the Himalayan Mountains, and of the Flora of Cashmere. By J. FORBES ROYLE, Esq. F. L. S. & G. S. &c. of the Hon. East India Company's Establishment, 393
2. An Outline of the Geology of Norfolk. By SAMUEL WOODWARD, Member of the Yorkshire Philosophical Society, and Author of a Synoptical Table of British Organic Remains, 394
3. Entomologia Edinensis; or, a Description and History of the Insects found in the neighbourhood of Edinburgh. By JAMES WILSON, F. R. S. E., M. W. S. &c.; and the Rev. JAMES DUNCAN, M. W. S. - - - 394

ART. XXIII. Proceedings of the Society for the Encouragement of the Useful Arts in Scotland, - 396

XXIV. List of Patents granted in Scotland from 23d September 1833 to 19th March 1834, 397

NOTICE TO CORRESPONDENTS.

Dr THOMSON'S Chemical Account of a New Mineral—Mr EISDALE'S Memoir on Ground-Ice—Mr MEIKLE'S Observations on the Hygrometer—Analysis of the *New* Craigleith Fossil Tree, &c. &c. in our next.

CORRIGENDA.

In Dr DAVY'S paper on Phosphorus, in Vol. XV.

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

HISTORICAL ELOGE OF ALEXANDER VOLTA. *By M. ARAGO, Permanent Secretary to the Academy of Sciences.*[*]

When yellow amber is rubbed, it has a strong attraction for light bodies, such as the barbs of feathers, pieces of straw, and saw-dust. Theophrastus among the Greeks, and Pliny among the Romans, had observed this property, but without appearing to attach to it a greater degree of importance than to a simple accident of form or colour. They did not suspect that they had touched the first link of a long chain of discoveries, and were little aware of the importance of an observation, which, in more recent times, furnished the means of disarming the stormy clouds, and conducting into the bowels of the earth, without even the danger of an explosion, the thunder with which these clouds are charged.

The Greek name of amber, *electron*, has led to the word electricity, which at first denoted the powerful attraction of bodies subjected to friction. The same word, however, is applied to a great variety of effects, and to all the details of a noble science. Electricity remained for a long time in the hands of physicians, the almost exclusive result of complicated combinations, which natural phenomena rarely presented united. The man of genius, whose works I am this day to analyze, was the first who went beyond these narrow limits. By means of some microscopic apparatus, he ascertained the existence of electricity almost everywhere, in combustion, in evaporation, and even in the simple touch of two dissimilar bodies. He thus assigned to this powerful agent, an importance among terrestrial phenomena scarcely inferior to that of gravitation. The connection of these important discoveries appears to me deserving of being traced and illustrated. At a period when the need of accurate knowledge is so generally felt, I conceive that these academical eloges may become the landmarks to a general history of the sciences. To the present attempt to contribute to their usefulness, I freely invite the severe and enlightened criticism of the public.

---

[*] This interesting Biography or Eloge was sent to us from Paris, by Mr Pentland.

ALEXANDER VOLTA, one of the eight foreign associates of the Academy of Sciences, was born at Como, in Milan, on the 18th February 1745. He was the son of Philippe Volta and Madeleine de Conti Inzaghi. His early studies were carried on under the eye of his father, in the public school of his native town. His natural endowments, steady application, and love of order, soon placed him at the head of his school-fellows. At eighteen years of age, the studious scholar had already commenced a correspondence with Nollet on some of the most complicated questions in physics. At nineteen he composed a Latin poem, not yet published, in which he described the phenomena discovered by the most celebrated experimentalists of the times. It has been said that up to this period Volta had not decided on his profession, but this I am inclined to doubt. A young man who has thought of making chemistry the subject of his literary compositions, would not long hesitate in favour of poetry. If we except a few verses celebrating Saussure's journey to the top of Mont Blanc, we never find the illustrious philosopher engaged in works of any other kind than those devoted to the study of nature.

Volta had the boldness at the age of twenty-four, and in his first work, to touch upon the very delicate question of the Leyden Phial. This apparatus was discovered in 1746. The singularity of its effects would have been amply sufficient to justify the curiosity it excited throughout Europe; but this curiosity was greatly increased by the foolish exaggeration of Muschembroek, and by his unaccountable terror on receiving a very slight shock; to which, he said emphatically, he would not again expose himself for the fairest kingdom in the world. It would now be quite superfluous to collect the various theories to which the phial gave rise. The honour of having solved the important problem is due to Franklin, and the work of Volta, it must be allowed, has added very little to the knowledge we derived from that of the illustrious philosopher of America. The second memoir of the philosopher of Como appeared in 1771. Here we scarcely find any idea of a systematic kind. Observation is the author's only guide in the researches which he undertakes in order to determine the nature of the electricity of bodies with various coatings,—to assign the circumstances of temperature, of colour, of elasticity, which cause the phenomena to vary,—to study electricity, whether produced by friction, by percussion, or by pressure,—or that which is engendered by the aid of the file or the grater,—and, finally, to ascertain the properties of a new kind of electrical machine, in which the moveable table and the insulated supports were of dried wood.

On this side the Alps, the two first memoirs of Volta were scarcely read. In Italy, on the contrary, they produced a lively sensation. Authority, whose predilections are so generally misplaced, particularly where, in its blind love for absolute power, it refuses to competent judges the right of decision, was eager in this case to encourage the young experimentalist. He was nominated superintendent of the Royal School of Como, and soon after Professor of Natural Philosophy.

The missionaries of Pekin, in the year 1755, communicated to the learned of Europe an important fact which they had accidentally observed, concerning electricity, whose influence on certain bodies appears or disappears according as these bodies are separated or brought into contact. This fact gave rise to the interesting researches of Æpinus, Wilcke, Cigna, and Beccaria. Volta, in his turn, made it the subject of particular study, and found in it the germ of his *perpetual electrophorus*, an admirable instrument, which, though of the smallest volume, is an inexhaustible source of electrical fluid, from which charges of equal power can always be obtained without recourse being had to friction, and in all states of the atmosphere.

In 1778 another very important work succeeded the memoir on the Electrophorus. It was already known, that a given body, whether empty or full, has the same electrical capacity, provided the surface continue uniform. An observation of Lemonnier indicated, that, besides equality of surface, the form of the body is not without influence. Volta, however, was the first to establish this principle on a solid foundation. His experiments shewed, that of two cylinders of the same surface, the longest received the strongest charge; a great advantage would therefore arise from substituting for the large cylinders of ordinary machines, whenever circumstances permitted, a series of very small cylinders, although their collective mass should not be of larger size. In combining, for example, sixteen rows of slender rods silvered over, each 1000 feet in length, we should possess, according to Volta, a machine the sparks of which, like real thunder, would destroy the largest animals.

Not one of the Professor of Como's discoveries was the fruit of chance. All the instruments with which he has enriched science, existed in principle in his imagination before an artist attempted to execute them. There was nothing fortuitous, for instance, in the modifications to which Volta subjected the electrophorus in order to transform it into a condensator;—that singular microscope, as it may be called, which reveals the presence of the electrical fluid, when no other means could accomplish it.

In the years 1776 and 1777, Volta wrought for some months at a subject of pure chemistry; but, at the same time, electricity, the science to which he was so much attached, will be seen to mix with his researches in the most fortunate combinations.

At this time chemists had found native inflammable gas only in mines of coal and rock-salt; they therefore regarded it as one of the exclusive attributes of the mineral kingdom. Volta, whose attention had been drawn to the subject by an accidental observation of P. Campi, shewed that they were mistaken. He proved that the putrefaction of animal and vegetable substances is always accompanied by the production of inflammable gas; and that if the bottom of stagnant water, or the mud of a lake, be stirred, the gas escapes through the water, producing all the ordinary appearances of ebullition. Thus the origin of the inflammable gas of marshes, a subject which had so long occupied the attention of chemists, was a discovery of Volta.

This discovery produced the belief that certain natural phenomena, such, for example, as inflammable earth and burning fountains, were owing to a similar cause; but Volta knew well how far nature sports with our weak conceptions, to give way to mere analogies on slight grounds. He visited the celebrated districts of *Pietra Mala* and *Velleja*; he carefully weighed all that he read in books of travels relating to analogous places, and at last established, on the most satisfactory evidence, in opposition to the ordinary opinion, that these phenomena do not depend on the presence of petroleum, naphtha, or bitumen; but that a disengagement of inflammable gas is the only cause. I am of opinion, however, that we may be permitted to doubt, whether Volta has proved so satisfactorily that this gas originates, in all cases, in a maceration of animal and vegetable substances.

Our illustrious associate possessed, in a high degree, two qualities which are rarely found united, a creative genius, and the power of assiduous application. He never abandoned a subject without having regarded it in all its aspects, and having described, or at least indicated, the various instruments which science, industry, or mere curiosity, might derive from it. Thus some experiments regarding the inflammability of the air in marshes, gave rise first to the electrical gun and pistol, of which it would be superfluous to speak, since they have passed from philosophers into the hands of mountebanks, and form the daily amazement of the crowds in our public places; secondly, the *perpetual lamp of oxygen gas*, so general in Germany, and which, by a most ingenious application of the electrophorus, kindles of its own accord when wanted; and finally, the *Eudiometer*, a valuable means of analysis which chemists apply to so many useful purposes.

The discovery of the composition of atmospheric air has given rise, in our time, to this important question in natural philosophy;—whether the proportion in which the two principal constituent principles of air are united, varies with the succession of ages, the position of places, and with the change of seasons?

When we reflect that men, quadrupeds, and birds, continually consume, in the act of respiration, one of these principles only, viz. the oxygen gas; that this same gas is the indispensable aliment of combustion in our domestic fires, in workhouses, and furnaces; that not a candle nor a lamp can be lighted, nor a fire of any kind, without absorbing it; and that oxygen, in fine, occupies so important a place in the phenomena of vegetation,—we may imagine that in course of time the atmosphere varies considerably in its composition; that it will one day be unfit for respiration; that then all animals will be annihilated, not in consequence of one of those physical revolutions of which geologists have found so many indications, and which, in spite of their immense extent, may leave a chance of safety to some individuals favourably situated; but by a general and inevitable cause, against which the frozen zones of the poles, the burning regions of the equator, the immensity of the ocean, the prodigiously elevated plains of Asia and America, the snowy summits of the Cordilleras and of the Himalaya, would be equally unavailing. To study all the particulars, as well as the actual period of that grand phenomenon, to collect the exact data that future ages will supply, is the task that philosophers have been anxious to accomplish, particularly since the eudiometer has given them the means of doing it. To obviate some objections to which the first trial of this instrument gave rise, MM. de Humboldt and Gay-Lussac submitted it to the most scrupulous examination. When such judges declare that none of the eudiometers approach in accuracy to that of Volta, we are no longer at liberty to doubt the fact.

Since I have abandoned chronological order, before occupying myself with the most important works of our venerable confederate, before analysing his researches regarding atmospheric electricity, and characterizing the discovery of the pile, I shall mention in few words the experiments that he published in the year 1793, on the expansion of air.

This important question had attracted the attention of a great number of able natural philosophers, who did not agree either upon the total increase of volume the air undergoes between the temperatures of melting ice and the boiling point; nor upon the gradations of expansion in the intermediate temperatures. Volta discovered the cause of these disagreements: he shewed that in operating in a vase containing water, increasing expansions should be found; that if there is no other humidity than that with which vitreous partitions are generally covered, the apparent expansion of the air should increase in the lower parts of the thermometric scale, and decrease in the elevated degrees. He proved, in short, by delicate measurements, that atmospheric air, if shut up in a vase perfectly dry, dilates in proportion to its temperature, when this is measured upon a mercurial thermometer equally divided: now, as the works of Deluc and Crawford appear to establish that a similar thermometer gives the true measurements of the quantity of heat, Volta thought himself authorised to announce the very simple law which flowed from his experiments, in these new terms, of which every one will appreciate the importance;—that the elasticity of a volume of air is proportionate to its heat.

Air on being heated when at a low temperature, and containing the same quantity of moisture, has its elastic force augmented like dry air. Volta thence concluded that steam and air expand in the same degree. Every one now knows that this result is correct; but Volta's experiment left some reason to doubt; for at ordinary temperatures, the vapour of water mingles with atmospheric air in very small proportions.

The work which I have just analyzed, which Volta called a mere rough draught, he intended to incorporate with many other researches of the same kind, in a memoir which was never published. Our knowledge of the subject is now complete, owing to the exertions of Gay-Lussac, and Dalton. The experiments of these ingenious chemists, made at a time when the memoir of Volta, although published, was not known either in France or England, shewed that the law laid down by the learned Italian extended to all other gases.

I shall not examine the researches of Volta on atmospheric electricity, until I have taken a rapid survey of analogous experiments that preceded his. In order to judge correctly of the route a traveller has traversed, it is often useful to see likewise the point from which he set out.

Dr Wall, who wrote in 1708, should be first mentioned, for in one of his memoirs this ingenious reflection occurs: "The light and crackling noise of electrified bodies, seem, in a certain degree, to represent thunder and lightning." Stephen Grey published, in 1735, a similar remark. "It is probable," says this illustrious natural philosopher, "that in time means will be found of concentrating large quantities of electrical fire, and of increasing the power of an agent which, according to many of my experiments, if I may be allowed to compare great things with small, appears to be of the same nature as thunder and lightning."

Many have seen nothing in these passages but mere comparisons. They do not believe that in likening the effects of electricity to those of thunder, Wall and Grey intended to infer an identity of causes. This doubt, however, cannot be entertained with regard to the views inserted by Nollet, in 1746, in his lessons on experimental philosophy. Here a stormy cloud, above terrestrial objects, is regarded by the author as nothing else than an electrified body, placed in presence of bodies which are not so. *Thunder in the hands of Nature, is electricity in the hands of philosophers.* Many resemblances of actions are noted; nothing in short is wanting to this ingenious theory but the sanction of direct experiment.

The first views of Franklin on the analogy between electricity and lightning were, like those of Nollet, mere conjectures. All the difference between them consisted in a plan of experiment, of which Nollet had not spoken, and which promised definite arguments for or against the hypothesis. In this experiment the trial is to be made during a storm, to determine whether a metallic rod, insulated and terminating in a point, does not give sparks analogous to those which arise from the conductor of an ordinary electrical machine.

Without attempting to detract from the fame of Franklin, I must remark that the proposed experiment was almost unnecessary. It was witnessed during the African war by the fifth Roman legion, when, according to Cæsar's account, the iron of the javelins appeared to be on fire at the close of a storm. Numerous navigators have likewise seen it in the phenomenon named *Castor and Pollux*, either on the metallic points of their masts and yards, or on other projecting parts of their ships. Finally, in certain countries, in Frioul for instance, at the Castle of Duino, the sentinel did exactly what Franklin wished, when, in order to determine when it was necessary to ring a bell to warn the country people of the approach of a storm, he went to examine with his halbert if the iron of a pike placed vertically on the rampart, produced sparks. It is to our countryman Dalibard, however, that science is indebted for direct experiment on the subject, by which he dispelled every remain ing doubt. On the 10th May 1752, during a storm, the large pointed rod of metal, which he had placed in a garden at Marly-la-ville, produced small sparks, like the conductor of an electrical machine when an iron wire is applied to it. Franklin did not realise this experiment in the United States, by means of a kite, till a month later. *Paratonnerres* or *conductors* were the immediate result of this discovery, which the illustrious American hastened to proclaim to the world.

That part of the public which is reduced to the necessity of judging of matters of science on hearsay, seldom pronounce on the merits of any discovery by halves. They admit or reject, if I may so speak, with passionate eagerness. Conductors, for example, became the objects of a remarkable enthusiasm, the traces of which it is curious to remark in the writings of the time. Here you will find travellers who, in a flat country, imagine they can conjure the lightning by raising their sword against the clouds, in the attitude of Ajax threatening the gods; there ecclesiastics, whose costume does not admit of a sword, bitterly regret being deprived of this talismanic preserver; one seriously proposes as an infallible preservative, to place one's self under a gutter, seeing that wet cloth is an excellent conductor of electricity; while another invents certain head-dresses, from which are suspended long metallic chains, which care must be taken to drag in the water, &c. &c. Some men of science, it must be acknowledged, did not participate in this absurdity. They admitted the identity of lightning and the electrical fluid, the experiment of Marly-la-ville having afforded decisive proof of this; but the sparks emitted by the rod were so few and small, that there was doubt whether it were possible to draw off by this means the immense quantity of fulminating matter with which a cloud must be charged. The experiment made by Romas de Nerac, did not overcome their opposition, because this observer had employed a kite with a metallic cord, which reached the region of the clouds. Soon, however, the lamentable death of Richman (on the 6th August 1753), occasioned by a simple discharge from an insulated bar, which this distinguished man had placed on his house at St Petersburgh, threw new light on the subject. In his tragical end, the learned saw an explanation of a passage, in which

Pliny the naturalist relates, that Tullus Hostilius was struck with lightning, for having carelessly performed the ceremonies by means of which Numa, his predecessor, had caused the thunder to descend from heaven. On the other hand, some imagined that they had found in the same occurrence something not previously known, viz. that in certain circumstances a bar of metal, somewhat elevated, attracts from the clouds not only imperceptible sparks, but what may be called streams of electricity. The discussions subsequent to this period on the efficacy of conducting rods, are destitute of interest. I do not except from this character the warm debates which for some time divided the learned in England, on the conductors terminating in a point or in a ball. No one is now ignorant that George III. was the promoter of this dispute; that he declared for conductors terminating in a ball; while Franklin, his antagonist in political questions of immense importance, wished them to end in a point. This discussion, therefore, properly considered, is rather a trifling incident in the history of the American revolution, than one connected with science.

The results of the experiment at Marly were scarcely known, when Lemonnier, of this Academy, erected in his garden at St Germain-en-Laye, a long bar of metal, in a vertical position, which he insulated with greater precaution than before; and from that moment the electrical spark appeared not only when the thunder was heard, and the atmosphere full of threatening clouds, but even *when the sky was perfectly serene.* A beautiful discovery thus resulted from a modification, apparently of the most insignificant kind, of the apparatus first used by Dalibard.

Lemonnier ascertained that this lightning in a clear sky, the existence of which he had just discovered, was liable during the whole twenty-four hours to regular variations in its intensity. Beccaria made some excellent observations on the laws of this diurnal period, and established the important fact, that in all seasons, at all heights, and during the prevalence of every wind, the electricity of a clear sky is constantly positive or vitreous.

Following in regular order the progress of our knowledge in atmospheric electricity, I now arrive at the works with which Volta has enriched this important branch of meteorology. His labours have been directed to the improvement of the means of observation, and the minute examination of the different circumstances under which the electrical fluid is developed, which thus pervades all the regions of the air.

When a branch of science is in its infancy, observers are chiefly occupied in discovering new phenomena, reserving the estimate of their importance to another period. In electricity, many individuals had obtained a well merited reputation; the Leyden phial adorned all the cabinets of Europe; but no one had hitherto thought of a true electrometer. The first instrument of this kind is not of earlier date than 1749: it was made by two members of this Academy, Darcy and Le Roy; but its want of mobility under small charges, prevented its adoption.

The electrometer proposed by Nollet (1752) appeared at first to be more simple and convenient, while it was of infinitely greater sensibility. It was composed of two wires, which, having been electrified, could not fail to open, by an effect of repulsion, like the two arms of a compass. The measure sought was thus reduced to the observation of an angle.

Cavallo realized what Nollet had merely indicated (1780). His threads were of metal, and supported at their extremities small balls made of the pith of the alder-tree.

Volta at last laid aside the pith, and substituted dry straws for the metallic threads. This change would appear unimportant, were it not mentioned that the new electrometer possessed the valuable and unexpected property, of producing between 0° and 30° angular dispositions of the two straws exactly proportionate to the electrical charges.

The letter to Lichtenberg, of the date 1786, in which Volta established by numerous experiments the properties of straw electrometers, contains many interesting views on the means of rendering these instruments comparable, on the measure of the strongest charges, and on certain combinations of the electrometer and condensator, of which it is surprising that no notice is taken in any of the more recent works. This letter cannot be too much recommended to young natural philosophers. It will initiate them in the difficult art of experimenting; it will teach them to distrust first appearances, and to vary incessantly the form of trial; and if a warm imagination induce them to abandon the slow but certain path of observation, for the allurements of speculative reveries, they will perhaps be led to resist this deceptive influence, by observing a man of genius submitting to the most laborious details. At a time, moreover, when the publication of a book, except in a few honourable instances, is a purely mercantile operation; when treatises on science, in particular, are cut to the same pattern, and present scarcely any appreciable shade of difference; when each author scrupulously neglects all experiments, theories, and instruments, which his immediate predecessor has overlooked or misunderstood, I conceive it a duty to direct beginners to original sources. By this means alone will they be led to important subjects of research, and find a faithful history of discoveries; while they will learn at the same time to distinguish between truth and uncertainty, and to distrust conjectural theories which undiscerning compilators adopt with blind confidence.

When Saussure had taken advantage of the powerful effect which points exercise on the electrical fluid, and had greatly augmented the sensibility of Cavallo's electrometer, by the simple addition of a stalk of eight or nine decimeters in length; and when the metallic threads with balls of alder pith had been superseded by dry straws, it might have been supposed that the apparatus was not susceptible of further improvement. In 1787, however, Volta increased its power considerably, without making any change in its primitive construction. This he effected by a most singular expedient, that of fitting to the point of the metallic stalk introduced by Saussure, either a wax-light, or merely a burning match!

The result of this experiment surely no one could have foreseen. Experimenters were not slow in discovering that flame is an excellent conductor of electricity, but ought not this fact to have discouraged the idea of employing it as a collecting power? Volta's soundness of judgment and severe logic, prevented him from giving way to the consequences of the strange fact that had occurred to him, till he had found an explanation. He perceived that if a light brings to the point where it is placed, three or four times more electricity than could otherwise be collected, it must be owing to the current of air produced by the flame, and the multiplied communications thus established between the metal point and the atmospheric molecules.

Since flame subtracts electricity from the air much more readily than pointed metallic rods, does it not follow, said Volta, that the best means of preventing storms, or diminishing their destructive effects, is to kindle large fires in the fields, or, what is still better, on elevated situations? After reflecting on the powerful effects of the minute wick attached to the electrometer, it seems no way unreasonable to suppose that a large flame may deprive, in a few moments, great bodies of air and vapour, of all their electrical fluid.

Volta was desirous to submit this opinion to the test of direct experiment. Hitherto his wishes have not been carried into effect. Perhaps they would be looked upon in a favourable point of view, if a comparison were instituted between the meteorological observations made in the counties of England, where great furnaces transform the night and day into oceans of fire, and those of the neighbouring agricultural counties.

These *feux paratonnerres* caused Volta to lay aside the severe gravity of his ordinary manner. He tried to sport with his subject at the expense of some learned individuals, who, like the famous Dutens, always perceived, when too late, the discoveries of their contemporaries in some ancient author. He called upon them, in this case, to go back to the fabulous times of the Greeks and Romans, and directed their attention to the sacrifices under the open sky, to the flames ascending from the altars, and the black columns of smoke rising into the air from the bodies of the victims; to all the ceremonies in short which the vulgar believed fitted to appease the wrath of the gods, and disarm the fulminating hand of Jupiter. All these were only a simple experiment in physics, of which the priests alone possessed the secret, designed to bring silently to the earth the electricity of the air and clouds. The Greeks and Romans, at the most brilliant periods of their history, offered sacrifices, it is true, in covered temples; but, adds Volta, "this difficulty admits of a reply, since it may be said that Pythagoras, Aristotle, Cicero, Pliny, and Seneca, were only ignorant people, who had not acquired even by tradition, the scientific knowledge of their ancestors!"

No criticism could be more effective; but in order to expect some good result from it, it is necessary to forget, that in seeking in old books for the rudiments of great discoveries, the Zoili of all ages are less anxious to honour the dead, than to detract from the reputation of their cotemporaries.

Almost all natural philosophers attributed these electrical phenomena to two fluids of a different nature, which, in certain circumstances, accumulate separately on the surface of bodies. This hypothesis naturally led them to seek for the source of atmospheric electricity. The problem was an important one, and a delicate, though simple, experiment led to its solution.

In this experiment, an insulated vase, from which water evaporated, gave out, by the aid of Volta's condensator, evident marks of negative electricity.

I regret that I am unable to determine with certainty to whom this capital experiment is due. In one of his memoirs, Volta mentions that it had been present to his mind from the year 1778, but that various circumstances prevented him from attempting it, till the month of March 1780, when it was successfully performed by him at Paris, *in company* with some members of the Academy of Sciences. On the other hand, Lavoisier and Laplace, at the close of the memoir which they published on the same subject, merely say that "Volta shewed anxiety to assist at our experiments, and to render himself useful."

How can two accounts so contradictory be reconciled? A historical note published by Volta himself, is far from removing our doubts. It is neither expressly stated in whom the idea of the experiment originated, nor which of the three observers suggested that it would succeed with the aid of the condensator. The first trial made at Paris by Volta and the two French savants, proved abortive, the state of the atmosphere not being favourable. A few days after, at the country residence of Lavoisier, the electrical signs became manifest, although the means of observation had not been altered. Volta was not present at the last trial.

This circumstance has occasioned all the difficulty. Some, without farther examination, consider as inventors those who are the first to establish a fact by means of experiment. Others regard this as a very secondary merit, and little else than a mechanical operation; they therefore bestow their approbation on the original projectors.

Both these principles are too exclusive. Pascal entrusted to his brother-in-law Perrier, the task of ascending the Puy-de-Dome to observe the barometer, and yet the name of Pascal is the only one associated with that of Toricelli, when we speak of the weight of the atmosphere. Michell and Cavendish, on the contrary, divide with no others the merit of their celebrated experiment regarding the attraction of terrestrial bodies, although many before them had thought of performing it; in this instance the execution was all. The labours of Volta, Lavoisier, and Laplace, are not exactly parallel to either of these cases. I admit that a man of genius might conceive, without suggestions from another, that electricity concurred in the production of vapours; but in order to bring this idea out of the region of hypothesis, it was necessary to establish particular means of observation, and even to construct new instruments. Those employed by Lavoisier and Laplace, they owed to Volta. They were made at Paris under his inspection, and he assisted at their first trial. Such abundant proofs of direct co-operation, indisputably connect the name of Volta with every theory of the electricity of vapours; but who will presume to affirm, in the absence of a positive declaration from himself to the contrary, that the experiment was not undertaken at the suggestion of the French philosophers? In this state of doubt, will it not be most judicious, on this as well as on the other side of the Alps, no longer to separate, when speaking of these phenomena, the names of Volta, Lavoisier, and Laplace, and to cease to make the question a source of mutual recriminations, which scarcely admit of an excuse, even if the truth were obvious.

These reflections will terminate, I trust, an unpleasant discussion which malignant passions are so prone to perpetuate; they will afford, at all events, an additional proof how delicate a matter is the just appropriation of intellectual labour. When three of the most highly gifted men of the eighteenth century, arrived at the height of reputation, could not agree as to the share of invention due to each in an experiment made in common, need it excite our surprise to witness similar disputes among those who are just entering upon their career?

Notwithstanding the length of this digression, I cannot leave the experiment which has led to it, without shewing its importance as the foundation of a very curious branch of meteorology. For this purpose a few words will suffice.

When the insulated metallic vase in which the water evaporates, becomes electrical*, it is because this water, in order to pass from the liquid to the aëriform state, derives from the bodies which it touches, not only heat but likewise electricity. The electrical fluid is therefore an integral part of the great masses of vapour which are daily formed at the expense of the water of the sea, lakes, and rivers. These vapours, rising into the higher regions of the atmosphere, there meet with a cold which condenses them. Their electrical fluid is disengaged, accumulates, and the weak conductibility of the air prevents it returning to the earth, whence it originated, unless accompanied with rain, snow, and hail, or in violent discharges.

Thus, according to this theory, the electrical fluid, which, in a stormy day, darts its dazzling flash through every quarter of the sky,—which produces such resounding explosions, and, when falling on the earth, carries along with it conflagration and death,—is the product of the daily evaporation of water, the inevitable consequence of a phenomenon developed in such imperceptible shades that our senses cannot mark its progress! When effects are compared with causes, what singular contrasts must Nature be confessed to present to our contemplation!

I now arrive at one of those rare epochs, in which a remarkable and unexpected fact, usually the result of some fortunate accident, is matured by genius, and becomes the cause of a revolution in science. The detailed picture of important results brought about by trifling causes, would not perhaps be less interesting in the history of the sciences than in that of nations. If any one undertake the delineation, that branch of physics known under the name of Galvanism, will occupy one of the most conspicuous places. It may, in fact, be proved, that the immortal discovery of the pile is connected, in the most direct manner, with a slight cold caught by a Bolognese lady, in 1790, and a dish of frog-soup prescribed by the doctor as a cure.

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* It is now known that the experiment does not succeed when distilled water is employed. This circumstance, certainly a very curious one in relation to the theory of evaporation, detracts nothing from the importance, in a meteorological point of view, of the labours of Lavoisier, Volta, and Laplace, since the waters of the sea, lakes, and rivers, are never perfectly pure.

Some of these animals, already stripped of their skin by Madame Galvani's cook, were lying on a table, when an electrical machine was accidentally discharged at a distance. The muscles contracted strongly when the electrical spark issued, although it did not reach them. The experiment, on being repeated, succeeded equally well with every kind of animal, with artificial or natural electricity, either positive or negative.

This phenomenon was very simple. If it had presented itself to some skilful natural philosopher, familiar with the properties of the electrical fluid, it would scarcely have attracted his attention. The extreme sensibility of the frog as an electroscope, might indeed have been the subject of some remarks, but it would have been carried no further. Happily, and by a rare exception, deficiency of knowledge in this case became profitable. Galvani, though an expert anatomist, knew little of electricity. The muscular movements which he had observed, appeared to him inexplicable, and he thought himself transported to a new world. He hastened to vary his experiments in every possible manner. In this way he discovered the singular fact, that the limbs of a frog, long after it has been decapitated, undergo great contractions, without the intervention of any foreign electricity, when a metallic plate, or, what is still better, two plates of different kinds of metal, are interposed between a muscle and a nerve. The astonishment of the Professor of Bologna was therefore perfectly warrantable, and he shared it with all the rest of Europe.

An experiment which caused the legs, thighs, and trunk of an animal which had for many hours been torn in pieces, undergo great convulsions, leap to a distance, and appear restored to life, could not continue long insulated. By analyzing it in all its details, Galvani imagined that he perceived in it the effects of a Leyden phial. According to him, animals are, as it were, reservoirs of electrical fluid. The positive electricity has its seat in the nerves, and the negative electricity in the muscles. With regard to the metallic plate placed between these organs, it was merely the conductor by which the discharge was effected.

These views seduced the public; they were adopted by physiologists, and electricity dethroned the nervous fluid, which had occupied such a prominent place in explaining the phenomena of life, although, by a strange oversight, no one had attempted to prove its existence. They flattered themselves, in a word, that they had discovered the physical agent which conveys external impressions to the *sensorium;* which arranges the organs of animals in the order of their intelligence; and produces the motions of the arms, limbs, head, and whatever the will has determined. These illusions, alas! were to be of short duration; the pleasing romance disappeared before the rigorous investigations of Volta. This ingenious philosopher produced convulsions, not by interposing two different metals between a muscle and a nerve, as Galvani had done, but by causing them touch a muscle only.

From that moment, the Leyden jar could no longer be appealed to for an explanation. The negative electricity of the muscles, and the positive electricity of the nerves, were pure hypotheses, without foundation; the phenomena resembled nothing previously known, and were covered with a veil that seemed impenetrable.

Volta, in the mean time, was not discouraged. He imagined that he had found electricity to be the principle of convulsions; that the muscle was entirely passive, and ought to be considered merely a conductor by which the discharge was effected. With regard to the electrical fluid, Volta had the boldness to suppose that it was the necessary result of the contact *of two metals,* between which the muscle was compressed; I say of two metals, and not of two plates; for, according to Volta, without a difference in the *nature* of the two bodies in contact, no electricity could be evolved.

The natural philosophers of every country in Europe, as well as Volta himself, adopted at first the views of the discoverer of galvanism. They agreed in considering the spasmodic convulsions of dead animals as one of the greatest discoveries of modern times. A very slight knowledge of the human heart may lead us to suspect, that a theory designed to connect these curious phenomena with the ordinary laws of electricity, would not be admitted by Galvani, and his disciples, without great reluctance. In truth, the Bolognese school eagerly defended the doctrine of animal electricity, which had become so widely diffused without opposition.

Among the numerous facts which this celebrated school opposed to Volta, there is one so singular as to throw us for a moment into a state of suspense,—I mean the convulsions which Galvani produced by touching the muscles of a frog with two plates, not of different kinds, as Volta believed necessary, but both taken from the same piece of metal. This effect, although it was not constant, presents in appearance an insurmountable objection to the new theory.

Volta replied, that the plates employed by his opponents might be identical in the name they bore, and even in their chemical nature, and yet differ in other circumstances, so as to possess properties truly distinct. In his hands, indeed, the plates, although composed of two contiguous portions of the same piece of metal, acquired a certain power when they had changed their temperature, degree of nealing, or even the polish of one of their elements.

This discussion, therefore, did not invalidate the theory of the celebrated professor. It proved only that the word *dissimilar*, applied to two superimposed metallic elements, had been understood in relation to electrical phenomena in too restricted a sense.

Volta had to sustain another and a more severe assault. Dr Valli, his antagonist, had produced convulsions simply by the touch of two different parts of the frog, without the use of metal, which, in all analogous experiments, had been, according to our associate, the principal generator of electricity.

It may be perceived, from more than one passage in Volta's letters, how much he was hurt by the tone of assurance with which the galvanists, old and young (I use his own expressions), boasted of having reduced him to silence. This silence, at all events, did not long continue. An attentive examination of Valli's experiments soon convinced Volta that there were two conditions necessary to secure success,—as great a heterogeneity as possible between the organs of the animal brought into contact, and the interposition of a third substance between these organs. The fundamental principle of the Voltaic theory, instead of being endangered, thus acquired additional extension. The metals no longer formed a class by themselves, and analogy led to the belief that two dissimilar substances, of what nature soever they might be, would produce, by mere touch, an evolution of electricity.

The attacks of the galvanists were not now formidable. They no longer confined their experiments to the smaller animals. They produced singular nervous motions in the nostrils, tongue, and eyes of an ox, which had been a long time dead, strengthening, more or less, the expectations of those in whose eyes galvanism seemed a means of reanimating the dead. On the theory itself, they threw no additional light. In drawing their arguments, not from the nature, but from the greatness of effects, the adepts of the Bolognese school resembled a certain philosopher, who, in order to prove that the atmosphere is not the cause of the rise of the mercury in the barometer, thought of substituting a wide cylinder for the narrow tube of that instrument, and then represented, as a formidable difficulty, the exact number of quintals of liquid raised.

Volta had given a death-blow to animal electricity. His conceptions always adapted themselves to experiments, which, being ill understood, were thought likely to afford the means of refutation; and he could not obtain the unreserved assent of all men of science. The contact of two metals, of two dissimilar substances, gave birth to a certain agent, which, like electricity, produced spasmodic movements. Of this fact there could be no doubt; but was the agent in question really electrical? Were the proofs adduced sufficient to prove it so?

When two metals of different kinds, are placed on the tongue, in a certain order, an acid taste is felt at the moment of their contact. If the position of these metals is changed, an alkaline taste is perceptible. On applying the tongue to the conductor of an ordinary electrical machine, a taste is felt, of an acid or alkaline nature, according as the conductor is electrified in a greater or less degree. In this case, the phenomenon is unquestionably owing to electricity. Is it not natural, said Volta, to infer identity of causes from similarity of effects; to liken the first experiment to the second; and to see no other difference between them, but the mode in which the fluid is produced which stimulates the organ of taste?

No one will dispute the importance of this agreement. The penetrating genius of Volta saw in it enough to warrant a full conviction. Ordinary minds had to demand more explicit proofs. These proofs, in the form of indisputable demonstration, before which all opposition vanishes, Volta found in a capital experiment, which I shall now briefly explain.

Two polished disks of copper and zinc are applied face to face, without any thing between, and attached to insulating handles. By means of these handles, the disks are drawn rapidly apart; they are then presented, one after the other, to the ordinary condensator, provided with an electrometer, and, mark the result, *the straws diverge immediately!* By this it is likewise shewn, that the two metals are in opposite states of electricity, the zinc being positive and the copper negative. By bringing the two disks repeatedly into contact, Volta succeeded, as with an ordinary machine, in producing bright sparks.

After this experiment, nothing remained to be done regarding the theory of galvanic phenomena. The production of electricity by the simple contact of two dissimilar metals, assumed its place among the most important and best established facts in physical science. If at that period any wish regarding it was unsatisfied, it was that ready means might be discovered of increasing this kind of electricity. Such means are at the present day in the hands of every experimenter, and it is to the genius of Volta that we are indebted for the possession of them.

In the beginning of the year 1800 (the date of such a discovery ought to be carefully noted), it occurred to the illustrious Professor, in consequence of some theoretical views he entertained, to form a long column, by placing above each other in succession a round plate of copper, another of zinc, and a piece of moist woollen cloth, taking care never to invert this order. What might have been expected *a priori* from such a combination? I hesitate not to assert, that this apparently inert mass, this fantastical assemblage, this *pile* of metallic pieces, separated from each other by a little liquid, forms, with regard to the singularity of its effects, the most wonderful instrument which man has ever invented, without excepting the telescope and the steam-engine.

I shall be acquitted, I feel assured, of all reproach of exaggeration, if, in the enumeration which I am about to make of Volta's apparatus, I am at the same time permitted to mention the properties which he himself discovered, and those that we owe to his successors.

It will readily be perceived, from the little that I have said regarding the composition of the pile, that the two extremities are necessarily dissimilar; that is, if there be zinc at the base, there will be copper at the top, and *vice versâ*. These two extremities are called the *poles*.

Let us suppose that two wires are attached to the opposite poles, copper and zinc, of a voltaic pile. The apparatus, in this form, is adapted to the different experiments which I wish to enumerate. When a person takes hold of one of these wires, he is not sensible of any effect; but no sooner does he touch both than he experiences a violent shock. This, it will be seen, is the phenomenon of the famous Leyden jar, which, in 1746, excited to such a degree the admiration of Europe. But the jar could be used only once, and required to be recharged after every trial. The pile, on the contrary, yielded a thousand shocks in succession. It may therefore be compared to the Leyden jar in the nature of its effects, if the latter suddenly recovered its first condition after each discharge.

If the wire which is attached to the zinc pole be placed on the tip of the tongue, and the wire of the copper pole on another part of the tongue, a very decided acid taste will be felt. In order to vary the nature of this taste, and make it become alkaline, it is necessary only to change the situation of the two wires.

The sense of sight is not exempted from the influence of this protean instrument. Here the phenomenon appears the more singular, as the luminous sensation is excited without the necessity of touching the eye. When the end of one of the wires is applied to the forehead, the cheeks, the nose, the chin, or even the throat, at the instant the observer seizes the other wire with his hand, he perceives, although his eyes are closed, a light which varies in form and brilliancy according to the part of the face the electrical fluid attacks.

By analogous combinations sounds are produced in the ear.

It is not on organs in a healthy state alone that the pile acts; it excites, and seems to reanimate, those in which life appears quite extinct. Under the combined action of the two wires, the muscles of the head of a hanged criminal underwent such terrible contortions, that the spectators fled in dismay. Sometimes the trunk of the victim partly rose up, the hands were tossed about, and struck objects near them, and even raised weights of several pounds. The muscles of the breast imitated the movements of respiration; and all the motions were so like those of a living being, that one felt inclined to ask, whether the experimenter was not adding to the sufferings which the law had inflicted on the criminal.

Insects, likewise, when subjected to the same trial, shew some curious results. The wires of the pile, for example, increase greatly the light of the kinds that are luminous; they restore the motions of a dead grasshopper, and cause it to sing.

On leaving the consideration of the physiological properties of the pile, and regarding it as an electrical machine, we shall be transferred to that department of science which Nicholson and Carlisle, Hisinger and Berzelius, Davy, Oersted, and Ampere, have cultivated with such brilliant success.

Each of the wires, considered by itself, is of the ordinary temperature, that is, the temperature of the surrounding air. At the moment the wires touch each other, they acquire a strong heat; when rather fine, they become incandescent; and if still finer, they are completely melted, and run like a liquid, even though they be formed of platina, the most infusible of known metals. With a very strong pile, two slender wires of gold or platina undergo, at the moment of their contact, a complete vaporization, and disappear like a thin vapour.

Pieces of charcoal, fitted to the two extremities of the same wires, kindle as soon as they touch each other. The light they diffuse is so pure, sparkling, and remarkable for its whiteness, that we should not surpass the limits of truth by calling it the solar light.

And who can say that this analogy ought not to be carried still farther, or whether the experiment does not solve the most important problem in natural philosophy; revealing the secret of that peculiar kind of combustion which the sun undergoes for so many ages without any sensible diminution either of matter or of splendour? The charcoal attached to the two wires of the pile, become, in fact, incandescent in a perfect vacuum. Nothing, therefore, can be incorporated in their substance, and nothing appears to emanate from them. At the close of an experiment, however long continued, the pieces of charcoal are found to be the same in nature and weight as before it began.

Every one knows that platina, gold, copper, &c. have no perceptible influence on the magnetic needle. Wires of these metals attached to the two poles of the pile, are in the same predicament, if taken singly; but the moment they are brought in contact, a very strong magnetic action takes place. Nay, during the whole time they continue in contact, these wires are themselves true magnets; they charge filings of iron, and communicate a permanent magnetism to plates of steel placed in their neighbourhood.

When the pile is very strong, and the wires are at some distance from each other, a bright flame unites their extremities. This light is magnetic; a magnet attracts and repels it. If Franklin and Coulomb, without previous preparation for such a disclosure, were to hear me speak of a flame attractable by a magnet, a lively feeling of incredulity would be the most favourable sentiment with which I could expect to be regarded.

Let the same wires, at some distance from each other, be plunged into a liquid, into pure water, for example. The water will be instantly decomposed; the two gaseous elements which form it will be separated; and the oxygen will be disengaged by the wire attached to the zinc pole, and the hydrogen, remote from that point, by the wire at the pole of copper. In rising to the surface, the globules of gas do not quit the wire by which they were developed; the two constituent gases may therefore be collected in two separate vessels.

When a liquid holding saline substances in solution, is substituted for water, these substances are analyzed by the pile. The acids are conveyed to the zinc pole, and the alkalies incrust the pole of copper.

This means of analysis is the most powerful known, and has recently enriched science with a multitude of important results. It is to the pile, for instance, that we owe the decomposition of numerous alkalies and earths, which were previously regarded as simple substances; it is by the pile that all these bodies are derived from oxides; that chemistry now possesses metals, such as potassium, which can be moulded by the fingers like wax, which are lighter than water, and therefore float on its surface, which kindle spontaneously, and diffuse the most brilliant light.

This would be the place to bring together all that is mysterious, I may say incomprehensible, in the decompositions effected by the voltaic pile; to insist on the separate disengagements of two gaseous elements from a liquid; on the precipitations of the principal solid constituents of the same saline molecule, which are effected in points of the dissolving fluid very remote from each other; and on the singular carrying or transporting power which these different phenomena appear to imply; but this would occupy too much time. Before leaving the subject, however, I must remark, that the pile does not act as a means of analysis only; if in changing to such an extent the electrical relations of the elements of bodies, it often occasions their complete separation, a judicious management of its power is become, in the hands of one of our Associates, the principal generator of numerous combinations of a most remarkable nature, and which art has hitherto been unable to imitate.

I said some time ago, gentlemen, with some diffidence, that the pile is the most wonderful instrument which human intelligence has ever constructed. If the enumeration which I have made to you of its various properties has at all done justice to the subject, I may now revert to my assertion, and regard it as fully established.

According to some biographers, Volta had so exhausted the energies of his mind by his long continued labours, particularly by the construction of the pile, that he was incapable of any new exertion. Others have regarded an obstinate silence of nearly thirty years' duration, as the effect of a puerile timidity from which this illustrious man was unable to free himself. He was apprehensive, they say, lest by comparing his new researches with the important discoveries he had already made in electricity, his mental vigour should be thought diminished. These two explanations are doubtless very ingenious, but they have the defect of being entirely futile; the date of the pile is in reality 1800; but two ingenious memoirs, one of them on the *Phenomena of Hail*, the other on the *Periodicity of Storms and the*

Cold which accompanies them, were not published till between sixteen and seventeen years after.

Gentlemen, I have now given you a delineation of Volta's brilliant career. I have attempted to characterize the great discoveries with which his powerful genius has enriched the physical sciences. It remains for me now, conformably to the usual practice, to recount briefly the principal occurrences of his private and public life.

The painful duties which devolved on Volta when scarcely beyond the years of childhood, detained him in his native town till 1777. This year he left, for the first time, the picturesque banks of the lake of Como, and traversed Switzerland. His absence did not continue for more than a few weeks, nor was it marked by any important scientific research. At Berne he visited the illustrious Haller, whom an immoderate use of opium was bringing rapidly to the grave. From thence he repaired to Ferney, where every species of merit was sure to meet with a kind reception. Our immortal compatriot, in the course of the long conversation which he held with the young professor, discussed the rich and varied branches of Italian literature; and spoke of the numerous philosophers, poets, sculptors, and painters of which this literature can boast, with such enlightened views, delicacy of taste, and soundness of judgment, as to leave an indelible impression on Volta's mind.

At Geneva, Volta formed an intimate friendship with the celebrated historian of the Alps, than whom no one was more able to appreciate his discoveries.

This was, indeed, an enviable age, gentlemen, when a traveller, in the course of a single journey, and without losing sight of the Jura, could pay homage to Saussure, to Haller, to Jean-Jacques, and to Voltaire.

Volta returned to Italy by Aigue-Belle, conveying to his fellow-citizens that valuable tuberous root, the proper cultivation of which renders famine almost an impossible occurrence. In Lombardy, where dreadful storms destroy the crops of an extensive country in a few minutes, an alimentary substance which grows and ripens in the bosom of the earth, secure from the ravages of the hail, was an invaluable boon to the inhabitants.

Volta had himself written a detailed account of his journey in

Switzerland, but it was laid up in the archives of Lombardy. Its recent publication was owing to a practice which, to all appearance, will not be soon adopted in certain countries, where, without being stoned, a writer can call marriage the most serious of buffooneries.

In Italy, where this ceremony is looked upon with more seriousness, every one, according to his station in life, is anxious to signalize it by conferring some favour on his fellow-citizens. It was on the occasion of the marriage of M. Antoine Reina, of Milan, in 1827, that this little work of Volta's was brought out from among the official records—those catacombs in which, in every country, so many treasures are for ever buried.

Such is the singular nature of human institutions, that the destiny of one of the greatest geniuses of which Italy could boast, was at the mercy of the Administrator-general of Lombardy. In choosing this functionary it was necessary, I suppose, that he should possess certain notions of finance, and that degree of rank which etiquette imperiously required: and such was the man who had irrevocably to decide, whether Volta deserved to be transported to a wider theatre, or should continue confined to the little school of Como, deprived all his life of expensive apparatus, which, if it does not supply the want of genius, at least renders it more efficient. In the case of Volta, chance corrected the inconvenience arising from such a state of dependence. The Administrator, Count de Firmian, was a friend of literature, and the school of Pavia having become the object of his particular care, he established in it a chair of natural philosophy, which in 1779 Volta was invited to fill. There, during a long period of years, a multitude of youth from every country eagerly attended on his instructions; there they learned, I shall not say the details of science, for almost every book gives these, but the philosophical history of discoveries—the subtile corelations which escape the perception of common minds—and, what few have the power of delineating,—the progress of inventions.

The language of Volta was lucid, unpremeditated, sometimes animated, and always distinguished by modesty and urbanity. These qualities, when united to merit of the first order, are particularly attractive to youth. In Italy, where the imagination is so easily excited, they had produced a complete enthusiasm.

The desire of appearing in the world as a scholar of Volta, contributed in a great measure, for more than the third part of a century, to the great success of the University of Tésin.

The proverbial *far niente* of the Italians, is strictly true with regard to bodily exercise. They travel so little, that even in the wealthiest families, we find a Roman whom the sublime eruptions of Vesuvius have never drawn from the cool shade of his *villa;* well educated Florentines to whom St Peter and the Coliseum are known only by engravings; and Milanese who all their lifetime have believed on hearsay that there exists, at a distance of some leagues, an immense city, and hundreds of magnificent palaces built in the midst of the waves. Volta himself did not leave the banks of his native Lario, except for scientific purposes. I do not believe that his excursions in Italy ever extended to Rome or Naples. If, in 1780, we see him crossing the Apennines on his way from Bologna to Florence, it is with the hope of finding by the way, in the fires of *pietramala*, an opportunity of submitting to a decisive proof the ideas he entertained regarding the origin of native inflammable gas. If, in 1782, he visited, in company with the celebrated Scarpa, the capitals of Germany, Holland, England, and France, it was to form an acquaintance with Lichtenberg, Van-Marum, Priestley, Laplace, and Lavoisier, and to enrich the cabinet of Pavia with certain instruments, of which descriptions and figures could convey only an imperfect idea.

By the invitation of General Bonaparte, conqueror of Italy, Volta returned to Paris in 1801. He there repeated his experiments on electricity by contact, before a numerous commission of the Institute. The First Consul assisted in person at the meeting in which the commissioners rendered a detailed account of these remarkable phenomena. Their report was scarcely ended, when he proposed to bestow on Volta a gold medal, in testimony of the gratitude of the French savants. It was contrary to custom, and the established rules of the Academy, to accede to this proposal; but rules are made for ordinary circumstances, and the professor of Pavia had placed himself beyond these. The medal was therefore voted by acclamation; and as Bonaparte did nothing by halves, Volta received on the same day, a sum of 2000 crowns to defray the expenses of his journey. The foundation of a prize of 60,000 francs, to be awarded to the individual who shall give an impulse to the sciences of electricity or magnetism, similar to that which they first received from Franklin and Volta, is another proof of the enthusiasm which the Great Captain had felt. This impression was lasting, and the professor of Pavia became in the eyes of Napoleon the type of genius. He was speedily decorated with the cross of the Legion of Honour, and the Crown of Iron; nominated a member of the Italian Council, and raised to the dignity of Count and Senator of the Kingdom of Lombardy. When the Italian Institute presented itself at the palace, if Volta happened not to be among the foremost ranks, the abrupt questions, "Where is Volta? is he unwell? why is he not present?" showed perhaps too obviously, that, in the eyes of the sovereign, the other members, in spite of all their knowledge, were merely satellites to the inventor of the pile. "I cannot consent," said Napoleon in 1804, "to the retirement of Volta. If the duties of a professor fatigue him, he may diminish them. Let him deliver, if he please, only one lecture in the year, but the University of Pavia would be struck to the heart, were I to permit a name so illustrious to disappear from the list of her members; besides," he added, "a good general ought to die on the field of honour." The good general found the argument irresistible, and the Italian youth, whose idol he was, continued to enjoy his valuable instructions for some years.

Newton, during the course of his parliamentary career, is said to have spoken only once, and that was to ask an attendant on the House of Commons, to shut a window that admitted a current of air, and might give a cold to an orator then addressing the House. If the door-keepers at Lyon, during the Italian council, or those of the Senate at Milan, had been less careful, perhaps Volta's goodness of heart might have led him to overcome his extreme reserve, though it were only for an instant; but the opportunity was wanting, and the illustrious philosopher will inevitably be classed among those timid or indifferent persons, who live among revolutions and animated assemblies of people, without expressing an opinion or uttering a single word.

It has been said that happiness, like material bodies, is composed of imperceptible elements. If this idea of Franklin's be just, Volta was happy. Entirely devoted, in spite of his high political dignities, to the labours of the cabinet, nothing disturbed his tranquillity. Under the law of Solon he would even have been banished, for none of the parties which agitated Lombardy for nearly a quarter of a century, could boast of having him in their ranks. The name of the illustrious professor appeared only after the tempest, as it were for an ornament to the authorities of the day. In private, Volta shewed the greatest aversion to conversation relating to public affairs; he made no scruple, whenever an opportunity offered, of cutting it short by one of those playful words, known in Italy by the name *freddure*, and in France by that of *calembourg*. We must suppose that long practice in this did not render him infallible, for many of the *freddure* of the great philosopher, which some do not scruple to repeat, are far from being as irreproachable as his experiments.

Volta was married in 1794, at the age of forty-nine, to Therese Peregrini. He had three sons; two of these have survived, the other died at eighteen years of age, at the moment when the highest expectations were formed of him. This misfortune is, I believe, the only one that our philosopher experienced throughout his long career. His discoveries were no doubt too brilliant not to excite envy, but envy did not dare to attack them even under its most ordinary guise, for their novelty could not be disputed.

Discussions about priority have at all times been the bane of inventors. Hatred, for that is the feeling which usually gives rise to them, is not scrupulous in choosing the means of attack. When proofs are wanting, recourse is had to sarcasm, which is too often successful. It is said that Harvey, who had eagerly refuted the numerous criticisms to which his great discovery gave rise, completely lost courage when some of his adversaries declared, in the form of a concession, that they acknowledged him the merit of *having caused the circulation of the blood to circulate*. Let us congratulate ourselves, gentlemen, that Volta was never engaged in such debates; let us congratulate his countrymen for having saved him from them. The Bolognese school no doubt believed long in the existence of animal electricity. Honourable feelings of nationality led them to wish that the discovery of Galvani should remain entire; that it should not enter, as a particular case, into the great phenomena of voltaic electricity; yet, at the same time, they never spoke of the phenomena but with admiration—never did an Italian mouth pronounce the name of the inventor of the pile, without accompanying it with the most unequivocal marks of esteem and respect—without uniting it to a word of expressive simplicity, very endearing to the ears of a citizen, for never, from Rovérédo to Messina, do the educated inhabitants speak of the philosopher of Pavia, but as *our* Volta.

I have already mentioned the dignities conferred on him by Napoleon. All the great academies of Europe had already enrolled him among their numbers. He was one of the eight foreign associates of the first class of the Institute. So many honours never excited in Volta's mind one emotion of pride. The little town of Como continued to be his favourite abode. The tempting and repeated offers of Russia, could not induce him to change the pure sky of Milan for the fogs of the Newa.

A strong and rapid judgment, lofty and comprehensive ideas, and an affectionate and sincere disposition, were the dominating qualities of the professor's mind. Ambition, thirst of gold, and a spirit of rivalry, were not the springs of any of his actions. In him the love of study, which was the only passion he seems to have felt, was free from all worldly alliance.

Volta was of a lofty stature, his features noble and regular like those of an ancient statue, his forehead expansive, and deeply furrowed with laborious thought, and in his look tranquillity of heart and penetration of mind were equally depicted. His manners always retained traces of the rusticity contracted in his youth. Many recollect of seeing him when in Paris daily enter the bakers' shops, and purchase rolls of bread, which he eat as he went along the streets, without ever suspecting that he was doing any thing to attract observation. I shall be pardoned, I hope, for mentioning these minute particulars. Has not Fontenelle related that Newton had a thick head of hair; that he never used spectacles, and that he had lost only one tooth? So much do great names justify and ennoble the most trifling details!

When Volta quitted definitively in 1819, his charge in the University of Tésin, he retired to Como. From this period all his intercourse with the scientific world ceased. He scarcely admitted a few of the many strangers who came to pay their respects to him, attracted by his wide-spread reputation. In 1823, a slight attack of apoplexy brought on some alarming symptoms, but the prompt application of medicine succeeded in removing them. Four years afterwards, in 1827, about the beginning of March, the venerable old man was seized with a fever, which in a few days subdued his remaining strength. He died on the 5th of the same month without pain. He was then aged eighty-two years and fifteen days.

Como celebrated the obsequies of Volta with great pomp. The professors and students of the Lyceum, the friends of science, and all the enlightened inhabitants of the town and neighbourhood, hastened to accompany to their final resting-place the mortal remains of the illustrious philosopher, the virtuous father of a family, and the charitable citizen. The handsome monument which they have erected to his memory, near the picturesque village of Camnago, from which Volta's family originated, testifies, in the most conspicuous manner, the sincerity of their grief. All Italy participated in the sorrow of Milan; but on this side the Alps, a still more poignant sorrow prevailed. Such as are surprised at this assertion, have not observed that, on the same day, and almost at the same hour, France lost the author of the Mécanique Céleste. For six preceding years, Volta was dead to all but his own family. The names of the electrometer, condensator, and even of the pile itself, no longer retained the power of making his heart beat quicker! Laplace, on the contrary, preserved to the last that ardour and vivacity of mind, and impassioned zeal for scientific discoveries, which rendered him during half a century the heart of our meetings. When death surprised him at the age of seventy-eight years, he was publishing a continuation of the fifth volume of his great work. On reflecting on the greatness of this loss, it will be acknowledged, I doubt not, that it would be doing the Academy injustice to reproach her for having concentrated all her thoughts on the dismal loss she had sustained.—With regard to myself, gentlemen, my only apprehension is, that I have not succeeded to your wishes in evincing the immense services rendered to science by the illustrious Professor of Pavia. I flatter myself, at all events, that no one will accuse me of being insensible of their importance. Were we to pass in review all the works of our cotemporaries, and each, according to his habits, taste, and the direction of his mind, to determine of which he would prefer to be considered the author, the Mécanique celeste and the Voltaic pile, would present themselves at once to my thoughts! An academician devoted to the study of the stars, could not give a greater proof of the profound admiration with which the immortal discoveries of Volta have inspired him.

The place of Foreign Associate, left vacant by the death of Volta, has been filled up by Dr Thomas Young. Academic bodies are fortunate, gentlemen, when, in recruiting their numbers, they can find one genius to succeed another.

INSTRUCTIONS FOR OBSERVERS OF THE AURORA BOREALIS *.

Notwithstanding the attention which has been paid to the phenomena of the Aurora Borealis, and the various hypotheses which have been imagined to explain them, it will be found that there is a want of information on the points which are most necessary as bases of induction; and the British Association have therefore been induced to appoint a Committee, in the express view of directing observers to the really important features of this meteor; and of obtaining, by a system of contemporaneous observation, data which experience shews cannot be derived from insulated exertion.

Height of the Aurora Borealis.—The most important question respecting auroral phenomena, is their elevation above the earth's surface; and certainly (considering the easy process by which it may be determined) it must excite surprise to find, that while some observers assign to them a height of from 100 to 200 miles, others reduce it to 2, 3, or even less. Of the former, Dr Dalton and Mr Potter are the most distinguished; of the latter, Mr Farquharson of Aberdeenshire; while many have been unable to form any decided conclusion. Yet these different determinations lead obviously to as different theories of the Aurora: if the first be admitted, it can scarcely be an atmospheric phenomenon, as, *if air exist at that height*, its attenuation must be so great as to make its influence insensible, while the second would place it in the ordinary region of clouds. It is, however, possible that both may be correct, and that electrified clouds may assume luminous appearances in the lower regions of the atmosphere, (see the Report on Meteorology in the last volume of the British Association Reports, p. 255;) while a far greater height and different origin must be attributed to those faint nebulous bands, which are less frequently seen traversing the sky in a direction nearly at right angles to the magnetic meridian, appearing in the N. W. and (when they move) drifting to the S. E. with a slow regular motion. If *one* arch be visible at two distant places, as, for example, Edinburgh and Manchester, even without calculation we know that its height is very great, but it is asserted, that different observers are, in this case, observing different arches; on the other hand, it is replied, that if this were so, the intermediate arches must be visible, while, in the memorable instances of March 29. 1826, described by Dr Dalton, and September 29. 1828, observed by Mr Davies Gilbert, Captain Kater, Mr Harvey, &c., one only was seen in the sky. By the establishment of numerous stations, the Committee hope to obtain a decisive answer to this important question.

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* The above was circulated by the British Association for the Advancement of Science.

Sound of the Aurora Borealis.—It has been stated, that a sound accompanies brilliant displays of Aurora, especially in high latitudes; but here also, the evidence is of the most contradictory kind. In this country we can scarcely expect to resolve the doubt, as, except when the meteor is in our zenith, its distance is too great, on *any hypothesis* of its elevation, to allow a weak noise to be audible. It is, however, possible that this notice may reach observers more favourably situated. To obviate the influence of imagination, which has been supposed to be the principal agent in this observation, it may be mentioned, that, even where the meteor is but *one* or *two* miles distant, *five* or *ten* seconds must elapse between the corruscation and the sound supposed to be produced by it. Most writers, however, speak of them as contemporaneous. If the height of the Aurora exceed a few miles, sound can neither be excited in, nor transmitted by, the attenuated air.

Periods of the Aurora Borealis.—Another object for inquiry, is the existence of recurring periods of frequency and brilliancy in the Aurora. About fifty years since, its appearances were numerous and splendid; thirty years later they became more rare, till at length they were almost unseen in any part of Britain. This was particularly remarked in the Shetland Islands, where the inhabitants complained of the loss of this "useful light," and the necessity of substituting artificial illumination for ordinary farming purposes! Since that time the Aurora has reappeared, and perhaps in 1829 and 1830 may have reached a new maximum. It would be important to know whether similar changes have been noticed in America, and other parts of the globe.

Effect of the Aurora Borealis on the Magnetic Needle.—An opinion, founded probably on the resemblance between some electric phenomena and the Aurora, that this meteor exerts an influence on the magnetic needle, has long prevailed. This seems to be established by the numerous experiments of M. Arago, Mr Christie, and Sir John Franklin; but is opposed by the failure of Sir Edward Parry and others to detect such an effect. It is therefore a question, *under what circumstances* the needle is affected by it. Mr Farquharson (who considers the fact as unquestionable) is disposed to think it a necessary condition to its occurrence, that the streamers should reach the plane of the magnetic dip, that is, in Britain, a point in the magnetic meridian 70° alt. above the southern horizon. It may be added, that this action of the Aurora takes place when it is not *visible*; at Paris, where M. Arago detected it, this meteor is scarcely ever seen.

Mode of Observation recommended.—As uniformity of observation is essential to obtain the results desired by the Association, the following suggestions are offered to such gentlemen as may wish to take a share in the undertaking.

Each observer should, at 10 P. M., Greenwich time (which should be used throughout), examine the sky, and enter in a journal the Auroral phenomena that may occur, *as also the absence* of any, should that be the case. This examination should be continued till 11 *. It is also desirable that he should engage, as co-operators, any friends that may be situated a few miles to the north or south of his residence, as *their* observations, compared to *his*, are alone available for low Aurora; while *his*, combined with those made at more distant stations, will determine that of greater elevation. He will instruct them in the approximate methods of observation which follow.

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* It happens sometimes, that the most interesting appearances of the aurora begin and cease at earlier hours. It was particularly remarked at York,

Should Aurora be seen, among other objects of attention may be pointed out, its connection with the clouds, if any are visible, and its distance relative to them: the nature of the *black mass* sometimes seen under luminous arches, whether mere contrast with the enlightened part, or of a cloudy nature; its transparency or opacity, and the way in which it vanishes. If streamers appear; their breadth, apparent velocity, as determined by stars, their lateral motion, and their relation to arches or clouds, should be carefully observed. The southward motion of arches should also be determined, by observing the time between their passing over different stars; and any changes which may occur in their form noted, or any defect of symmetry. The determination of elevation can scarcely be applied to the streamers of Aurora, except when some sudden incurvation or change occurs, which may happen to be noticed at two stations; but the arches are of a less evanescent nature. For them the altitude above the horizon, and the azimuth with respect to the *true* meridian, are required. It must be observed, that the Aurora is not visible in telescopes, and therefore these elements are not susceptible of such precision, as to make large instruments necessary or even useful. A small theodolite, or one of the little altitude and azimuth circles, invented by Captain Kater, in which the telescope is replaced by a tube having two cross wires at one end, and a sight hole at the other, is all that is requisite. Having levelled the instrument, intersect with the wires the *lower edge of the arch* (which is always the best defined) *at its summit*, and read both circles. If the index corrections are not known, reverse and repeat the observation. The azimuth is the difference between the reading of the horizon circle, and that shewn when the sights are made to intersect a mark placed due north of the instrument's place, by any of the well known methods. It will *nearly* coincide with

on the 17th of September 1833, that the streamers had attained their greatest brilliancy, and the arches reached their highest elevation, before 9 P. M.

the direction of the magnetic needle; *but it is desirable to establish this as nearly as possible*, and for this we must know the summit of the arch more accurately than the unaided eye can give it. Intersect a part of the lower edge at a considerable distance from the vertex; read the horizon circle; turn the instrument round its vertical axis till it intersects the lower edge again; the half sum of this and the former reading, is the reading of the vertex. In these observations the *time* must be carefully noted at *each* intersection, and the error of the watch used, ascertained as soon as possible. Of course, those who are familiar with instruments will find no difficulty in this; but persons who depend for their time on dials, or even mail-coaches, are not to neglect the verification of this element. In the latter case, they will ascertain from what town the time is derived, and what error it is liable to; but it must always be remembered, that without a knowledge of the time, even though a rough one, all else is useless.

Very good observations may be made without divided instruments, by noting the position of the arch with respect to *three or more stars* at a *noted time*. By drawing a line on a celestial globe through them, the vertex of the arch may easily be found with sufficient accuracy, by those who are not familiar with spherical trigonometry, and, of course, its altitude and azimuth. Should the observation be made on a journey, the place of the road should be mentioned.

Any elevated object, as a spire, chimney, angle of a building, upright pole, &c., may be used for the same purpose. Let the observer change place with respect to it, till he sees the point of it that he has selected coinciding with the vertex of the arch; let him mark the position of his feet, so that he may recover it in the morning, and determine the *horizontal distance* of that point to the place where he stood, and its *elevation* above his eye; the latter divided by the former is the tangent of the altitude. If a theodolite can be procured, the altitude as well as the azimuth of this point may be determined at once.

Should the streamers have the appearance of converging to a point south of the zenith, the altitude and azimuth of that point should be determined. If possible the deviation and inclination of the magnetic needle should also be obtained.

These, owing to the imperfection of our magnetic instruments, are not easily obtained with accuracy, but there is little difficulty in tracing any *change* of the deviation produced by the Aurora with the requisite precision. A needle, suspended by a fibre of silk in a glass case, and playing against a scale of equal parts, will answer the purpose in default of a more complete apparatus. It should, however, be frequently examined, for (as has been stated) it often indicates the existence of Auroras not otherwise perceptible.

ON A FOSSIL TOOTH FOUND IN A RED SANDSTONE ABOVE THE COAL FORMATION IN BERWICKSHIRE. *In a Letter from Dr R. E. Grant.*

My Dear Sir,

I return you the interesting specimen of the Fossil Tooth in the Red Sandstone [*] you had the kindness to submit to my examination, and I am greatly obliged to you for the opportunity you have afforded me of forming my own opinion about a specimen, which is likely to be the subject of some controversy and speculation, in consequence of the judgment respecting it, pronounced by a most skilful and indefatigable anatomist, who has determined it to be the tooth of a wolf. As I am requested to state my opinion respecting the tooth, and as my examination of it has led me to a different conclusion from that of my able friend Mr Cliff, I feel called upon to justify that opinion, by stating the reasons that have led me to it. The white colour, the glistening surface, the conical and curved form, and the smoothness of the tooth, it is true, give it much the appearance of the canine tooth of a carnivorous quadruped, and its size and length I find to correspond nearly with that of the upper jaw of a wolf. But notwithstanding this general resemblance, on a closer examination the comparison entirely fails; the tooth of the wolf is proportionably thicker and more curved, and is less compressed, particularly in its cavity for the pulp. The tooth of the wolf, and of other carnivorous quadrupeds, is covered on its crown with a thick layer of enamel, which resists the knife like glass, and which retains extreme density in the oldest fossil specimens; the tooth in the rock is as soft as bone, and indeed you may cut it like a piece of gypsum with the softest knife. The tooth of the wolf and of other carnivorous quadrupeds, is never fluted around the base of the crown (the comparison is of course here made with the canine teeth); the fossil tooth is distinctly grooved around the outer margin of its base, and the rock is easily perceived to fill up these inequalities of its surface. At the base of the concave side of the fossil tooth, it expands more suddenly than that of a wolf, or similar carnivorous quadruped, but not more than teeth with which I shall presently compare it. Of all the teeth of carnivorous quadrupeds, the canine are the most variable, in the same species, in their form and development, the most similar in different species (and even in different orders of quadrupeds), and in the determination of fossil or existing animals, they afford the least satisfactory indications; next to them are the false molares; then the incisors; then the carnivorous tooth; and the most important and influential are the tuberculated molares, which are the most constant in their forms, present the greatest number of points for comparison, and are the most intimately related to the kind of food and the whole organization and habits of the animal. I am therefore far from placing any reliance on the determination of a species of these animals, founded on the inspection of a small part of the outer surface of the crown of a canine tooth imbedded in a hard sandstone rock, and this was all that was exhibited of the tooth, when Mr Cliff was in possession of the specimen, and gave his opinion about it.

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[*] This is the Fossil Tooth mentioned in our former Number, as having been found by General Lord Greenock, and which we considered as belonging to a fossil species of fish.—Edit.

I have removed the rock from the broadest part of the tooth and from its cavity, so as to obtain a full view of the transverse section of its base, the form of its cavity, and the structure and thickness of its parietes, so that I have had more points of comparison exposed, and more data on which to found my opinion. In all the teeth of quadrupeds approaching in form to this fossil tooth, there is a thick layer of enamel covering the crown, which is distinguished by its vitreous lustre, its translucency, its uniform compact texture, and its resistance to the point of the knife; and by these characters we can easily perceive the extent to which the enamelled coating passes down over the osseous substance of a recent or fossil tooth. I am well aware that there are many quadrupeds with angular teeth, where the enamel is found only on a small part of the surface, as in the incisors of Rodentia, &c., but none of these could be confounded with the tooth before me, which is obviously destitute of such a coating of enamel. Dr Prout, I understand, on his first inspection of the tooth, declared that it looked more like bone than enamel on the surface; and I am convinced, after careful examination, that his opinion is correct, and that the tooth belongs to that class of animals where these organs are generally composed throughout of a soft osseous substance, which yields readily, on the surface as well as in the centre, to the point of the knife. In no quadruped, recent or fossil, is there a tooth approaching to this in general form, in which a section of the base of the crown presents a series of curves extending round it, as you perceive in this fossil, but such a structure is very common both in reptiles and fishes. There is no tooth of this form in quadrupeds which are destitute of a root to fix it in its alveolus; but in this fossil you will perceive, by the groove which I have made in the rock at the base of the tooth, that there exists here no trace of a root in connexion with this crown. Would it be more philosophical to suppose that this tooth was that of a quadruped and once had a root, and that, before being imbedded in the soft sand which is now so hardened around it, it had been somehow neatly bisected at its thickest and strongest part, and only the crown is handed down to us; or that it belonged to a class of animals in which the teeth consist only of osseous crowns, without a trace of root, or any alveolus to receive it, as is the case with most reptiles, and almost all fishes? I purposely avoid adducing here any geological reasoning, which would materially support my view of this fossil, and wish to let the tooth speak entirely for itself, as a piece of anatomical structure. There is a great difference in the density both of the osseous substance and of the enamel of the teeth of quadrupeds, which is well known to those who are in the habit of cutting these substances to make artificial teeth, and as we pass through the cold-blooded vertebrata, the texture of both becomes softer, and the proportion of enamel becomes less, till it is entirely lost in fishes, where the teeth are often mere osseous spines, anchylosed to the supporting bone. Even the *osseous* part of a quadruped's tooth is denser than the surface of this fossil; but I find I can match it in softness by my specimens of the Ichthyosaurus from the Lias, in which the surface of the teeth can be grooved with the slightest pressure of the knife, as in this fossil from the sandstone, and indeed in those recent lacertine and ophidian reptiles, in which the teeth are, like those of fishes, without roots or alveoli. In looking at the glistening surface of the osseous teeth of these saurian reptiles, serpents and fishes, one would little expect that soft texture which we find upon trial; but their softness is admirably proportioned to the feeble attachment of their broad base, and to the necessity for their rapid growth and frequent renewal during life. I have compared this tooth with those of the larger loricated reptiles (crocodiles, gavials, and alligators); but besides the want of the root, which the teeth of these reptiles have, and the dense texture of these deeply fixed teeth, the fossil wants the longitudinal grooves with which these teeth are so constantly and minutely striated. It would be much more difficult to prove, that this is not the tooth of some of the Lacertida, where the teeth approach so near to those of fishes in their form and texture, and density, and want of roots, than to show that it is not like that of any known quadruped or loricated reptile. Taking the fossil tooth as an entire tooth, which has been preserved to us precisely as it fell from the jaw upon the loose sand, and I see no reason to doubt this; its tapering, conical and curved form; its soft and bony texture, its suddenly expanded base, the irregular projections and consequent waved outline of that base; its total want of a root to fix it in the jaw, and its wide rough cavity for the pulp at the base, are common characters of the teeth of a thousand osseous fishes at present inhabiting our seas, and have never yet been found, so far as I know, in the teeth of any recent or extinct quadruped. On a superficial inspection of the fossil tooth, any one might suppose (the form of the teeth of quadrupeds being more familiar to most than that of fishes), that it is simply the detached crown of a quadruped's canine tooth which has become softened by fossilization, and has acquired a fractured margin around its base, by long pressure during the consolidation of the imbedding sandstone. I have compared the density of this supposed enamel with that of the oldest enamelled surfaces of quadruped's teeth which I possess, from the diluvial caves and from the chalk rubble, where they have been exposed to much moisture, and I have not found the *enamel* reduced to this condition, although I have often found the *osseous* substance of such teeth reduced to a much softer condition, as in the ivory of mammoth's tusks. Besides, the walls of the fossil tooth are homogeneous in texture from the outer to the inner surface, and of equal density on both surfaces, which could not be the case, were it composed of a thick layer of enamel covering a softer sheath of bone, as in quadruped's teeth. With regard to the curves described by the margin of the exposed base of the fossil tooth, they require only to be examined with a lens to observe their smoothness and symmetry, and to see that they constitute a part of the original formation of the tooth, which would form a remarkably expanded base, were they drawn out to one regular curve. I send you some teeth of the common *Lophius piscatorius*, or angler-fish, which consist of a simple *osseous* crown, with an expanded corrugated base and wide rough cavity, nearly of the size and form of the fossil tooth, to show you in what manner that curved outline, so remarkable around the base of the fossil, is commonly produced in the teeth of fishes.

And now I have given you my principal anatomical reasons, for questioning whether this be really the tooth of a wolf or of any other quadruped imbedded in the sandstone rock, which is all that you have given me time to do. I should like to have had leisure to compare its form with that of a series of the teeth of the larger osseous fishes, where I have no doubt, from the infinite variety of forms they present, that a hundred forms like this could easily be found. Indeed, from the great variety of forms presented by the teeth of the same fish, I could nearly match this fossil from the jaws of the Lophius and Lepisosteus before me. The original soft and spongy fish-like condition of the base of the fossil tooth, is still indicated by the particles of sand which you may perceive have sunk into its substance, and from the extent to which the red oxide of iron has permeated and coloured that part of the tooth. The teeth of fishes have generally a dark colour in calcareous and argillaceous deposits, from their osseous texture and the abundance of animal matter in that bony substance, but in siliceous deposits like this, they have a lighter colour, perhaps from the quicker disappearance of the animal matter.

No one who has seen the imperfect condition of this relic, and understands the nature of the evidence it affords, would place much reliance on any attempt to identify it, or give confidence to any speculations founded on its determination; for one might, with almost equal show of plausibility, maintain it to be the claw of a crab, as the tooth of a wolf; and I have thrown out these doubts regarding the determination of this fossil, chiefly from observing the absurdities into which speculative geologists are sometimes led, by placing too much confidence in the determinations of imperfect remains, which do not present the means of their identification.—I remain, &c.

ROBERT E. GRANT.

23d August 1833.

THE RATTLE-SNAKE (*CROTALUS HORRIDUS*, LINN.) DISARMED BY THE LEAVES OF THE WHITE ASH (*FRAXINUS AMERICANA*, MICH.) *Communicated by Judge* WOODRUFF *to Professor* SILLIMAN.

DEAR SIR,—Last evening, while perusing your very interesting Journal, I found in vol. iii. p. 85, a communication to you by Professor Jacob Green, giving an account of a large quantity of *rattle-snake skeletons* found in a cave near Princeton College. Professor Green closes his communication with a passing notice of *a popular story* among the former inhabitants of that town, that the leaves of the white ash were obnoxious to these reptiles.

This brought to my recollection an occurrence connected with this subject, of which I was a witness; and I now proceed to relate it.

During the summer-months of 1801, I resided in the north-eastern part of the State of Ohio. Rattle-snakes were then very numerous in that region. I found the opinion universally prevalent among the inhabitants there, that the leaves of the white ash were highly offensive to the rattle-snake. Several persons of respectability assured me that the rattle-snake was never found on land where the white ash grows; that it was the uniform practice among hunters, as well as others, whose business led them to traverse the woods in the summer months, to stuff their shoes and boots, and frequently their pockets also, with white ash leaves, as a preventive of the bite of the rattle-snake; and that they had never known or heard of any person being bitten who had used this precaution.

Some time in the month of August, I went with Mr S. Kirtland and Dr C. Dutton, then residing at Poland, to the Mahoning, for the purpose of shooting deer, at a place where they were in the habit of coming into the river, to feed on the moss attached to the stones in the shoal-water. We took our watch station on an elevated part of the bank, fifteen or twenty yards from the edge of the water. About an hour after we had commenced our watch, instead of a deer, we discovered a large rattle-snake, which, as it appeared, had left his den, in the rocks beneath us, and was slowly advancing across a smooth narrow sand beach towards the water. Upon hearing our voices, or for some other cause, he stopped, and lay stretched out with his head near the water. It occurred to me, that an opportunity now offered to try the virtues of the white ash leaves. Requesting the gentlemen to keep, in my absence, a watch over our subject, I went immediately in search of the leaves, and on a piece of low ground, thirty or forty rods back from the river, I now found, and, by the aid of my hunting-knife, procured a small white ash-sapling, eight or ten feet in length; and, with a view to make the experiment more satisfactorily, I cut another sapling of the sugar maple, and with these wands returned to the scene of action. In order to cut off a retreat to his den, I approached the snake in his rear. As soon as I came within seven or eight feet of him, he quickly threw his body into a coil, elevated his head eight or ten inches, and brandishing his tongue, "gave note of preparation" for combat. I first presented him the white ash, placing the leaves upon his body. He instantly dropped his head to the ground, unfolded his coil, rolled over upon his back, writhed and twisted his whole body into every form but that of a coil, and appeared to be in great anguish. Satisfied with the trial thus far made, I laid by the white ash. The rattle-snake immediately *righted*, and placed himself in the same menacing attitude as before described. I now presented him the sugar maple. He lanced in a moment, striking his head into a tuft of the leaves, "with all the malice of the under fiends," and the next moment coiled and lanced again, darting his whole length at each effort with the swiftness of an arrow. After repeating this several times, I again presented him the white ash. He instantly stretched himself out on his back, and writhed his body in the same manner as at the first application. It was then proposed to try what effect might be produced upon his temper and carriage by a little beating with the white ash. This was administered. But instead of arousing him to resentment, it served only to increase his troubles. As the beating grew more severe, the snake frequently stuck his head into the sand as far as he could thrust it, seeming desirous to bore his way into the earth, and rid himself of his unwelcome visitors.

Being now convinced that the experiment was a satisfactory one, and fairly conducted on both sides, we deemed it ungenerous to take his life after he had contributed so much to gratify our curiosity; and so we took our leave of the rattle-snake, with feelings as friendly at least as those with which we commenced our acquaintance with him, and left him to return at leisure to his den.—*Amer. Journ. of Science & Arts*, vol. xxiii. No. 2. p. 337.

A SKETCH OF THE TERTIARY FORMATION IN THE PROVINCE OF GRANADA. *By* C. SILVERTOP, *Retired Brigadier in the Service of H. C. M., K. of the R. and D. O. of Charles III., and F. G. S. With Plates. Communicated by the Author.* (*Concluded from Volume XV. p.* 377.)

Cabo de Gata, San Pedro, La Carbonera.—In following the volcanic ridge lately alluded to, from its southern extremity at the Cabo de Gata to the village of Carbonera, a few miles beyond which it terminates towards the N.NE., tertiary beds are first met with at a locality called Roalquilar, where they form a short ridge of considerable elevation, and a few minor hills, composed of horizontal thick strata of quartzose freestone. Near the same locality, which is surrounded by trachytic hills, are excavations or quarries of great magnitude, worked perpendicularly, in a whitish-yellow argillaceous rock, intersected by several little metallic veins (sulphurets of copper and iron). It is probably a decomposed and altered feldspathic rock resting upon trachyte. No authentic history exists upon the origin of these excavations, but the neighbouring peasants attribute them to the Romans, who are supposed to have extracted alum from the rock. Soapstone, used in place of the article whose name it bears by the peasants, blood-red jasper quartz, chalcedony, agates, and amethysts, are found in the immediate neighbourhood. From this locality to the village of La Carbonera, a distance of about twenty miles, the tertiary formation may be followed,—in an irregular manner for the first four miles to the little seaport town of San Pedro,—but from the latter to La Carbonera almost continuously. Its greatest breadth along this line of coast appears to be about five miles. The deposit is variously composed of argillaceous marl, sandy loom, calcareous freestone, hard compacted quartzose freestone, loosely aggregated sandstone, and zoophytical limestone. In the immediate vicinity of San Pedro, a thick bed or deposit of horizontal strata of yellowish zoophytical limestone, has once formed an elevated and extensive table-formed tract, capping or overlying the rocks of the volcanic ridge which passes by this little sea-port, in its range along the coast from the Cabo de Gata to the village of La Carbonera. This tract has been partially denudated, as seen in section 8.; and the subjacent trachyte rock is exposed to view in a consequent hollow or depression, that separates a high tertiary hill, which extends for about three miles along the coast immediately to the north of San Pedro, from a minor eminence of the same deposit more retired from the shore.

The old castle and the little hamlet of San Pedro are built upon these tertiary beds, in a deep ravine or denudation between the high hill just alluded to, and another of the same order to the south of the village, the road or path to which from Roalquilar is carried along a slope of volcanic tuff, at the base of a high escarpment of the latter, to the Mediterranean Sea; See Sect. 9. Pl. III. This section presents one of many instances observed in the vicinity of Sad Pedro, of the immediate superposition of the tertiary beds to volcanic tuff, indicating that an alluvion had been formed on the subjacent trachytic rocks previous to the deposition of the tertiary beds. Analogous facts with respect to other rocks have been lately noticed.

In some strata of zoophytical yellowish limestone close to the village, Pectens, fragments of ostreæ and coralline remains, were observed, but the shells are so broken that few entire ones can be found.

In ascending the high table-formed hill to the north of San Pedro, over which an unfrequented road leads to the village of Carbonera, similar organic remains were frequently noticed. This elevated hill is entirely composed of yellowish zoophytical freestone, separated into thick horizontal strata, and presents to the Mediterranean a bold escarpment, that terminates in an undulating slope of volcanic tuff; Section 10. Pl. III. Its length is between two and three, its breadth about one and a half, miles, sloping down on its west side to a hollow, where denudation has uncovered the subjacent trachytic rocks. The area of its flat-shaped summit is barren, rugged, and uncultivated, but covered with esparto and other wild plants.

The descent on its northern end is rugged and precipitous, until, near the base of the hill (See Sect. 11.), the path gets upon the softer material, composed of the volcanic tufa. At the bottom of the descent there is a little hollow, or valley of denudation, watered by a small stream, beyond which the road again ascends, and enters upon a lower tertiary tract, intersected by several little dry valleys, which continue to border the coast from this point to La Carbonera. The subjacent trachytic rocks, however, are every now and then observed in the lower parts of this hilly ground. In one of the denudated hollows along which the road passes, inclosed between high sloping escarpments of tertiary beds, several fallen masses, consisting of alternating strata of semi-opal, or hydrate of silica, and coral-sand were observed, which I was prevented from tracing to their origin in consequence of the lateness of the hour. For the benefit of future observers, I must mention that the locality is between the northern end of the high table tract crossed in coming from San Pedro, and a remarkable insulated table-hill, near the coast, called La Mesa de Roldan.

Two or three miles beyond the locality just alluded to, several low escarpments, formed by alternating horizontal strata of coral sand, zoophytical freestone, and loosely aggregated quartzose sandstone, border the road to Carbonera, studded with tertiary testaceous remains of Balani, Echinus, Clypiaster, Ostreæ, and different species of Pectens, the latter in predominating numbers. There is subsequently a slight descent to the beach, which, bordered by the tertiary tract, is followed to the village of La Carbonera.

In a little hollow, or dry ravine, which terminates on the beach about a mile before reaching the village, a bed, or mass, of pearlstone, containing numerous imbedded nodules of dark bottle-green blackish obsidian, is observed, the upper part of the hilly ground on each side of the hollow being composed of the tertiary beds that are spread over an extensive area in the immediate vicinity of this little seaport. (See Sect. 12. Pl. III.)

La Carbonera.—This village is situated at the distance of about 100 yards from the beach, in a denudation of the tertiary tract that has been followed along the coast from San Pedro. The beds of this formation, which generally cover and conceal the subjacent volcanic rocks over a considerable area in their neighbourhood, extend along the coast about three miles beyond La Carbonera, when they abut against a ridge of mica-slate, called La Sierra di Cabrera, which descends to the beach, and forms the boundary to the former towards the north-east. It may therefore be stated, that they form a band of about twenty miles in length, and from four to six in breadth, which borders the Mediterrannean shore from the little seaport of San Pedro to La Sierra di Cabrera.

Constituting the high hill immediately to the north of the latter village, over which the road to Carbonera was followed, and which, from its long flat summit, is termed La Mesa (Table) de San Pedro, the remaining portion of the band forms a low broken hilly tract, characterised by one or two more elevated tabular eminences, and particularly by La Mesa de Roldan.

This tract presents, generally, a dry barren aspect, except in some little denudated hollows, where the decomposed materials of the tertiary beds, mixing with the detritus resulting from the decomposition of trachytic rocks, upon which the former rest, constitute a soil fit for cultivation. In no part of the line of coast followed from Malaga, eastwards, is the tertiary formation so well and extensively developed as in the neighbourhood of La Carbonera, and the high table-formed hills called La Mesa di San Pedro and La Mesa di Roldan, prove that it has once formed an extensive horizontal tract, of considerable elevation, the summit of the Mesa di San Pedro being between 500 and 600 feet above the level of the Mediterranean. The volcanic rocks upon which it has been deposited can also be traced from Roalquilar, where the appearance of tertiary beds was first noticed, to the base of the Sierra di Cabrera, and are therefore co-extensive with its range along the line of coast.

Careful and minute examination would, no doubt, discover in this tract a large series of tertiary organic remains, most interesting as evidence of the species that inhabited the Mediterranean Sea at that epoch, but a sufficient number were collected at different points between San Pedro and La Carbonera to prove the general nature of the deposit.*

Amongst different localities where they were observed in the vicinity of La Carbonera, one may be particularly mentioned. It is at the distance of about two miles inland from the village, in the hilly ground adjacent to the banks of a ravine, in which has been laid bare the subjacent trachytic rock. (See Sect. 13. Pl. I.) The banks are formed by masses of cinder-coloured trachyte, studded with small crystals of black volcanic hornblende. In ascending up its right bank over one of these masses, the first substance met with is laminated reddish sandy loam, the laminæ dipping at about 15° from the trachytic mass, no doubt in consequence of subsidence. The rising ground beyond this

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* M. Deshayes identified from this tertiary tract :

Pecten benedictus.	Pecten jacobeus.
striatus.	nov. spec.

Fragment qui pourrait appartenir au *Clipiaster alatus.*

The following were identified by Mr Sowerby amongst other fossils I procured at the same locality :

1. Echinus.	Balanus cylindricus ?
2. Clypeaster alatus.	Ostreæ.

And several undescribed species of Pectens.

loamy deposit is formed of a bed of calcareous sandstone, containing numerous pectens, and the summit of the contiguous hilly tract displays a bed of zoophytical limestone in thick strata, in which there is such an abundance of ostreæ, that a whole family of these molluscæ appears to have been entombed at this point. The strata of the calcareous sandstone and zoophytical limestone are horizontal.

Carbonera to Vera, distance five leagues. For the first three miles the road traverses the north-eastern portion of the tertiary band that has been followed along the coast from San Pedro, and then crosses La Sierra de Cabrera, at whose base the former has been stated to terminate. The breadth of this primary ridge, which is a ramification from the great Sierra Nevada chain, and descends to the shore, is here between eight and ten miles. It is composed of mica-slate intersected by quartz veins, in which I occasionally observed imperfect crystals of Andalusite. The road subsequently descends to the beach, along which it proceeds to a miserable village called La Garrucha, an open roadstead considered as the port of Vera.

From the Sierra de Cabrera to La Garrucha no tertiary beds were noticed: the coast in this interval is bordered by a broad expanse of shingle, beyond which inland succeeds the mountainous district belonging to the great primary chain that runs parallel to the Mediterranean. A village called Majacar stands upon one of these great groups of mountains, at about two miles distant from the shore.

Vera, a town of considerable importance, containing about 10,000 inhabitants, and the head-quarters of several marine departments connected with the military and fiscal duties of the adjacent coast, is situated at the distance of about four miles from La Garrucha, in an extensive open low tract, confined towards the SE. and NE. by districts of primary and transition mountains, presumed to belong to the tertiary formation. (See Sects. 14. and 15. Pl. I.) In passing from the Mediterranean shore near El Castillo de la Garrucha to Vera, the beach rises into an indurated mass of conglomerated shingle, which forms a bare inland escarpment, at the base of which there is a dead flat of argillaceous loam, succeeded by another flat gently rising inland, of yellowish micaceous sandy loam. The road goes over this tract for about two miles, crossing a stream in the latter part of it, in whose banks the sandy nature of the deposit is well seen. The rise of ground beyond the stream is a little more rapid, and shortly afterwards a soft sandstone in strata variously inclined, and a dark black-looking rock make their appearance, and for the next mile there is a semblance of alternation between these two substances; but a group of low mammelonated hills of pitchstone, almost contiguous to the road on the left or towards the west, explains the deceptive appearance. A portion of the pryogenic rock has burst through the sandy deposit along the line of road, and has probably been the agent of its consolidation, and of the variously inclined, and occasionally perpendicular, position of its strata. Although the sandstone strata are in places variously inclined, at high angles, and even perpendicular, the general dip is toward the north; and the last of them which are observed on the road to Vera, apparently beyond the influence of the rock that has distributed them so far, dip in the latter direction at about fifteen degrees. From this point to Vera the deposit again consists of sandy loam, but gradually acquires a little more elevation. A mile beyond the town, the open track to which the above observations apply, becomes contracted by the intervention of low hills, composed of alternating beds of sandstone, sandy loam and conglomerate, which dip at between 16° × 25° towards the north, and confined by primary and transition districts, is prolonged inland in a north-westerly direction, by Cantoria, Purchena, and Searon to the Basin of Baza, one of the chief inland towns of the province of Granada. This great fissure or aperture in the Sierra Nevada chain, is generally known by the appellation of the Valley of the Almanzora, and in that part of its course in which the villages of Uracal, Comartin, and Searon are situated, exhibits tracts of conglomerate and beds of micaceous sandy loam, in which Clypeaster alatus, Ostrea crassissima, Balani, and several species of Pectens, are met with. Another fissure, with a direction from S.S.W. to N.N.E. in the system of primary and transition rocks, that appear to have exclusively occupied, at some former period, the whole of this part of the Peninsula, connects the open tract in the vicinity of Vera with the plain of Lorca, in the province of Murcia, and it is filled up with a deposit similar to that near the former town. (See Section 16. Pl. I. shewing the fissure or sort of narrow dry valley, bounded by primary and transition mountains, between Vera and Lorca).

This argillaceous, marly, sandy deposit near Vera, in which the sandstone beds dip towards the north-west, underlies, according to fair geological deduction, the masses of conglomerate beds of micaceous sandy loam, containing tertiary organic remains in the higher part of the fissure or valley of the Almanzora just alluded to; and although the only zoological evidence of the epoch to which the former belongs, consists in the teeth of sharks found in an argillaceous mass near this town, I am inclined to refer it to the earlier part of the Spanish tertiary period, for these teeth are identical with those found in the lower tertiary bed or group; 1, at Alaurin near Malaga, and the testaceous remains found at Uracal, Comartin and Searon, are similar to those met with in the upper tertiary bed or group; 2, at the same locality.

In the group of low mammelonated hills of pitchstone contiguous to the road from La Garrucha to Vera, and to which the disturbance in the sandstone strata has been attributed, many instances are observed of an earthy whitish marlstone, changed into a porcelanic compact limestone of a brown and lavender colour, in alternating stripes or bands *, when in contact with the pitchstone. Other instances are seen where no change has taken place, but in different parts of the group pieces of whitish marlstone were observed interlocated in a mass of black pitchstone, in which several almond-shaped nodules of different sizes, but never bigger than an ordinary hen's egg, of crystallized carbonate of lime, are also embedded. The pitchstone is of a dark blackish colour, without any appearance of stratification, but separated by thin fissures or lines into fragments and polyhedral blocks: its fracture compact, somewhat splintery and inclined to the conchoidal, brittle in small, but tenacious and hard in large pieces.

This group occupies an area of about one mile and a half in length, and about one mile in breadth, and seems to form a link

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* Analogous facts with respect to limestone, when in contact with pyrogenic rocks, will be mentioned (at Cabero Negro, Monteagudo, and Orihuela), in a memoir upon the geological structure of the province of Murcia.

in the chain of volcanic action, which may be traced along the southern Mediterranean coast of Spain, from the Cabo de Gata to the Cabo de Palos, its eastern extremity. Other links in this chain will be subsequently mentioned; one of them, however, as it is within ten miles of Vera, must here be noticed. The road from this town to the city of Lorca, goes along the fissure or dry valley represented in Section 16; and about half way from Vera to a little village or hamlet called Pulpi, a group of conical hills of trachyte are observed at the base of the mountainous tract of mica-slate that confines the fissure towards the E. S. E.

The trachyte is of a light grey cinder colour, studded with small crystals of volcanic hornblende, has a semivitreous aspect, and is very hard and tenacious. It bears the greatest resemblance to the trachytic rock most common amongst the volcanic productions in the Cabo de Gata ridge.

I shall now conclude these few observations upon the geological appearances in the vicinity of Vera, by recording an historical fact probably not unconnected with the origin of these volcanic rocks, and which bears testimony to the operation of powerful subterranean agency, some three centuries ago.

Upon the summit of the conical hill (C.), Section 14. Pl. I. stood the ancient town of Vera, about half a mile distant from the site which the town occupies in the present day. This hill, formed of thick beds of conglomerate, was called El Cabezo del Espiritu Santo, and Vera la Viejo (old Vera), situated upon its summit, was utterly destroyed by an earthquake on the 9th of November 1518, the tabernacle of the Santissimo la Cramento alone being spared, according to legendary tradition. Several shocks had been previously felt, and the inhabitants had fled from the threatened town, but an interval of repose succeeding, returned to their dwellings. This respite was of short duration, for three days after they had resumed their usual avocations, the earthquake occurred which buried them all under the ruins of their town. The municipal authorities and a detachment of troops had left the town the preceding day, the former on a commission, the latter to garrison one of the forts upon the adjacent coast, and thus escaped the general destruction. The names of the members of the municipality, nine in number, are mentioned in the manuscript from which the above account of the melancholy fate of Vera la Viego was extracted.

Aguilas, Almazarron, Cartagena *.—Along the Mediterranean coast from Vera to Cartagena, the tertiary deposit presents no striking feature. It may, however, be traced in some little flats at the base of the primary and transition mountains that generally almost reach down to the beach, by the occasional appearance of its usual pectens and other shells, embedded in little scattered masses of gravel or sandy marl. Such are represented to be met with near Aguilas, in a memoir lately read to the Society by Captain Cooke, R. N., although they escaped my observation when I passed by this little seaport; but between Aguilas and Almazarron, and in the vicinity of the latter village, a few patches of the formation containing tertiary shells, were noticed. Almazarron may be cited as another locality where numerous mineral productions and volcanic rocks testify the direction of that line of subterranean agency, to which allusion was lately made, as will be more particularly mentioned in a subsequent memoir. Near Cartagena the tertiary formation is again well developed; but as it is here seen on the northern flank of the transition ridge that borders the Mediterranean, this locality is not exactly included in *the line of coast* which I have attempted to describe, but will be noticed in another memoir, in which I shall endeavour to point out the various tertiary deposits in, and the general geological structure of, the province of Murcia.

Explanation of Plate II. of former Volume, and Plate I. of present Volume.

Section 1. Represents the little tertiary tract contiguous to the upper part of the town of Malaga. Between Malaga and the Cementeri, Group 1. indicates the position of the lower or argillaceous bed of the tertiary deposit: between the Cementeri and the base of the mountainous country to the north, group 2, 2, 2, low hills capped by the superior tertiary beds.

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* The patches of the tertiary deposit along the coast line from Vera to Cartagena, belong geographically to the province of Murcia.

Section 2. Intersects the Valley of Malaga opposite Alaurin el Grande, and shews the position of the tertiary deposit close to this village. *a.* under the Sierra de Cartama indicates serpentine, which is seen under the highly crystalline limestone of this insolated ridge at its W. N. W. end.

Section 3. Shews the nature of the ground on the western bank of the Rio Guadamedina: the facts analogous to those of Section 1.

Section 4. Represents the superposition of the high table-formed eminences near the village of La Pizarra, composed of conglomerate, to the argillaceous or lower bed of the tertiary deposits, which may be followed up the valley from Malaga to this village, 25 miles distant.

Section 5. Shews the position of a patch of tertiary seen on ascending to the Convento de San Anton; 1. is grauwake-slates; 2. red sandstone; 3. secondary whitish-grey compact splintery nummulite limestone.

Section 6. An eminence of tertiary beds between El Castillo del Marquez and the Rio de Velez, on the road from Malaga to Velez-Malaga.

Section 7. Represents the low preserved tertiary tract that borders the Mediterranean between Almeria and Cabo de Gata, capped irregularly by beds of conglomerate in its higher part towards the Sierra de Alhamilla. The bed of innumerable garnets seen at the point indicated, have, no doubt, fallen out of decomposed mica-slate, which there forms the soil.

Sections 8, 9, and 10. Shew the horizontality of the tertiary beds near San Pedro, and their immediate superposition to trachyte, and trachytic tuff.

Section 11. Is longitudinal, and represents, as from the sea, the high tertiary hill called the Mesa de San Pedro, to the north of this little seaport, and a part of that towards its south. The waving line, under the horizontal ones expressing the tertiary beds, indicates the form which the trachytic tuff presents to the spectator from the sea, whose shore the base line is meant to exhibit. Amongst the lower tertiary hills on the right of the section, the fallen masses, composed of alternating strata of semi-opal and coral sand, alluded to in the Memoir, were found; the subjacent trachytic rocks occasionally come to clay in this tract, as indicated.

Section 12. Is also longitudinal, more or less parallel to the shore, and shews the little hollow or ravine in which the mass of pearlstone, with nodules of obsidian, is observed, at about one and a half miles from the village of La Carbonera. The tertiary tract terminates at C, the Sierra de Cabrera.

Section 13. is sufficiently explained in the memoir.

Sections 14. *and* 15. The latter is the prolongation of the former beyond Vera. A is the little group of mammelonated pitchstone hills, near the line of road from La Garrucha to Vera. B that part of the road where there is a semblance of alternation between sandstone strata and pitchstone.

Section 16. Represents the fissure or dry valley, which forms a communication between the open tract near Vera and the plain of Locca. The mountains on the right descend to the Mediterranean shore, some four or five miles distant, and in the border they form to the valley, is seen the little group of trachytic hills alluded to between Vera and Pulpi.

ON THE COLOUR OF THE ATMOSPHERE AND DEEP WATER. *By the Count* XAVIER DE MAISTRE. *(Concluded from former Volume*, p. 359.)

IT is requisite, in the first place, that the vein be sufficiently deep to absorb all the light transmitted by the skin, and that the latter should be of that degree of fineness necessary to transmit a great part of the light. If the vein is small, it reflects the colour of the blood, and becomes red *; and this colour, mingling with the opaline blue of the skin, forms those violet tints observed on the countenances of many persons. If the vein be still smaller, and lying nearer the epidermis, the transparency of the skin increases, and the red colour is fresher. Finally, a tissue of imperceptible veins, placed very near the surface of the skin, colours the lips and cheeks of young persons having a fine complexion, with a uniform tint of red; but, it may be observed, that these fine colours are not exactly of the same tint as the blood which produces them; they are mingled with the opaline blue, which renders the colour slightly carmine, sometimes inclining to purple and violet on the lips of persons of a sanguineous temperament.

Thus the differences in the size of the bloodvessels, and in their proximity to the epidermis, are sufficient to produce all the tints of blue, violet, red, and purple, observed on the human countenance, by means of the mixture of the opaline blue of the skin with the red of the blood.

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* It is in this way that the tube of a barometer of large size, and full of red-coloured liquid, appears black, while a tube of small dimensions, filled with the same liquid, is of a fine purple.

The red colour of the blood is not the cause of the blue tint of the veins; it might be black or green, without producing any change, it being sufficient that the colouring substance absorb all the light transmitted by the skin. This result may be obtained artificially by means of a very thin plate of ivory, which produces nearly the same effects as the skin. If one of its surfaces be spotted with ivory black, Prussian blue, cochineal, vert de vessie, prepared with much gum, and so thick as to be no longer transparent, these colouring substances will equally produce a blue tint on the opposite side, because they absorb all the light transmitted by the ivory. But if, instead of a colouring matter which absorbs the light, the experiment be performed with an opaque reflecting substance, the resulting tint is composed of opaline blue and the colour of the substance employed. The following are some examples of this.

The red oxide of lead (minium of painters), placed on an ivory plate, produces on the opposite surface a slight tint of carmine. Some painters avail themselves of this property of ivory in sketching the cheeks and lips of their miniatures, by applying a layer of minium on the corresponding parts of the opposite surface; an artifice by which the effect is more easily obtained, than by the direct application of a slight tint of carmine. But if Naples-yellow be used instead of minium, the opposite surface is greenish. We observe in these two examples that the opaline blue is mingled with the proper tint of the opaque reflecting body, while the blue alone appears when the colouring substance absorbs the light transmitted by the ivory.

The mixture of colours in oil painting, shews the production of opaline blue in a still more evident manner. The best known example is the mixture of white with vegetable charcoal, which produces a bluish tint. As indigo and Prussian blue in the mass approach to black, some have thought that the blue is a mixture of light and shade; but that the blue produced by the mixture alluded to above, is owing exclusively to the white and not to the black, is proved by the following process: two small boards are painted grey, one with a mixture of white lead and charcoal ground with oil, the other in covering *d'un glacis de charbon* the board previously prepared with white lead, in such a manner that both boards have the same shade of light. Their tint will be found different; the first will be bluish, and the second grey without any mixture of blue.

As transparent colouring substances lose in oil almost all the coloured reflection which they possess in a pulverulent state, and, therefore, approach to black in the mass, their mixture with white produces opaline blue, by which the natural tint of the colouring substance is modified.

All painters are aware of the striking difference which exists between the colour formed by the mixture of the lac of cochineal with white, and that which is produced by the same lac when spread in a thin coating over a surface prepared with white; the first is violet, and the second has all the purity and brightness of which this fine colour is susceptible. Artists, therefore, who wish to obtain the most beautiful cochineal red in their paintings, always use these lacs *en glacis*. Opaque and reflecting colouring substances, such as Naples-yellow, chromate of lead, and yellow ochre, produce, like white lead, an opaline blue by mixture with charcoal, and the effect is still more obvious. These composite colours, according to the theory, ought to produce only more or less obscure tints of yellow; there result from them, however, decided tints of green, which are often used to paint the herbage in landscapes; in this case it is the opaque colouring substance which is opaline.

I have mentioned the most remarkable effects of the singular property possessed by certain colouring substances, the mixture of which produces opaline blue; but there is a multitude of less obvious modifications resulting from the mixture of composite colours, which it is impossible to describe, but which may be always foreseen by the following statement: *When white lead, or opaque reflecting colouring substances, are mixed with black, or with transparent colouring substances, blue is produced, and the primitive tint of the colouring substance is consequently modified.*

These modifications are often very slight, but they never escape attentive observation.

In the observations I have made, I have spoken of well known facts, but such as appear to have no analogy with each other, and I think I have perceived that they all depend on the particular property of the blue ray, that, viz. of being reflected preferably to the other more or less refrangible rays, by the simple mechanical resistance of the molecules of the bodies which transmit the light. This resistance takes place in great masses of transparent fluids, such as the air mingled with the vapour of water, and water mingled with air. It takes place in opaque bodies, which are less transparent, but only when they are of small dimensions. Finally, it is observed in opaque white or coloured bodies, as in the fine skin which covers the veins, and in the mixture of colours.—*Bib. Un.* Nov. 1832.

GEOLOGY OF THE VALLEY OF OODIPOOR. *By JAMES HARDIE, Esq. Bengal Medical Establishment, Member of the Asiatic Society, &c. Communicated by the Author. (Continued from* p. 282. vol. xiv.

THE rocks found in the Valley of Oodipoor belong to the argillaceous schist series. They consist of a variety of argillaceous and talco-argillaceous schists, greenstones, greenstone schists, quartz-rocks, and limestones, or rather limestone schists. The great majority of these are distinctly stratified, the strata are always highly inclined, generally speaking vertical, or nearly so; in many situations they are much distorted, and local deviations from the general parallelism of the rocky beds are constantly observed. Some of these will be more particularly adverted to in the sequel; in the mean time, it may be laid down as a general rule, that the direction of the strata is from N. N. W. to S. S. E.

The rocks under review belong to a series very extensively distributed throughout this portion of India. In the south of Mewar, more especially, they form the hills and hill ranges of that wild and rugged tract, inhabited by the numerous tribes of Bheells, Colees, Meenahs, and Gapiahs, who, under one or other of these denominations, have for centuries led a life of professed plunderers and robbers. This series is generally disposed in reference to a central granite axis, and, in the instance under consideration, to the central granitic axis of the Aravulli mountain chain.

It does not appear that the granitic axis of the Aravulli can be traced continuously throughout the whole extent of this chain; as far as our knowledge enables us to speak, the line of continuity is frequently interrupted, but facts have been collected sufficient to satisfy us of the existence of such an axis, the most elevated point of which would appear to be the mountain Aboo, which rises to the height of 5000 feet above the sea.

The general direction of the Aravulli chain, from its commencement on the frontiers of Guzerat, to its termination on the borders of Dehlie, may be stated to be N. N. E. and S. S. W., or rather this range describes a curve having the above general direction. Towards the south, the strata have a decided bearing to the east of south; in the central regions, again, their direction is nearly north and south, and as we proceed north, they are observed trending to the east of north. These remarks, at least, are applicable to the rocky belts which occur to the eastward of the central ranges of the Aravulli. To the westward, other modifications may perhaps be discovered, as it does not necessarily follow that there should be a perfect parallelism between the strata forming the opposite flanks of a mountain mass so extensive. Proofs of this, though on a smaller scale, I shall have occasion to allude to in the sequel.

To the east and west of the Aravulli chain, the country has a gradual and almost imperceptible slope. On the one side, we have the districts bordering the Gulf of Kutch, the plains of Marwar, the arid districts of Jesselmere and Bicaneer, and the sandy desert. On the other, we have the modern district of Ajmer, Kishenghur, and Mewar; and further to the east, the principalities of Jepoor, Kotah, and Boondee; while to the south of these we have the table land of Malwa, composed of an enormous mass of overlying trap-rocks, the precise limits of which, to the eastward, have not as yet been correctly ascertained.

I have already stated that the strata of the Valley of Oodipoor bear N. N. W. and S. S. E. In this respect they are conformable to the formations which occur to the south, and deviate, to a certain extent, from the general line of bearing of the Aravulli chain. They dip, when not perfectly vertical, to the E. N. E. *.

Immediately to the eastward a series of primary rocks occur, of which gneiss associated with hornblende rocks, and rocks having the mineralogical characters of granite, are the preponderating members. This series flanks the argillaceous formations of the Valley of Oodipoor, and extends to the southward, forming the ranges in the neighbourhood of the Dhabur Lake. To the north it merges into the primary formations of the north of Mewar, and to the east is again succeeded by argillaceous schists and quartz-rocks, which dip under the sandstones of Malwa.

From the above distributions, it appears that the strata of the Valley of Oodipoor dip *towards* the primary rocks which occur to the eastward, to which latter they are conformable,—an arrangement, perhaps induced by the operation of an upheaving agency occurring to the westward, which was probably exerted with greatest energy in the neighbourhood of Aboo, but by no means inferring that the Oodipoor strata are inferior in the *original* order of superposition to the belt of rocks which flanks them to the eastward; but it is my object at present simply to record facts, and I shall reserve the little I have to say in the shape of comment, till such time as I shall have been enabled to furnish my readers with a connected view of the actual phenomena in their different bearings and relations to one another.

We shall now proceed to examine in detail the geology of the rocky belts above briefly adverted to; and, to render the account more connected and intelligible, we shall commence our survey from the plains of Mewar, bordering upon the Valley of Oodipoor. Taking our station at Meirta (lately the residence of the British agent at the Court of Oodipoor), situated on a rocky plain, about three miles to the eastward of the Dubaree Gate †, and the *bund* of the Oodisagor Lake, an opportunity is afforded us of examining the nature and structure of the belt of rocks of the granitic series, which I have alluded to as occurring in this position. In the neighbourhood of Meirta, the plain is elevated about 2000 feet above the level of the sea,—is very scantily covered with soil, and the strata are constantly observed protruding above the surface. The preponderating rocks have the mineralogical characters of true granites, but are laminated in strata generally of considerable thickness, or occur in the form of regular prismatic masses, alternating with the other strata. They vary considerably both in respect of texture and composition. Some are very coarse granular, others are of a much closer texture; a common variety is a *pegmatite* composed of large angulo-granular concretions of reddish felspar and translucent quartz, with scales of mica very sparingly interspersed; these are frequently altogether absent. This rock passes into a closer granular variety in which the mica is more abundant. Another variety is a *sienitic granite*, and this again passes insensibly into hornblende rocks, some kinds of which resemble the greenstones of the trap formation. The hornblende is occasionally replaced by chlorite or steatite, or these occur in the same mass with the other ingredients enumerated. Minute specks of a greenish mineral, apparently epidote, are very generally interspersed through the substance of all of the above varieties, and schiller spar also occurs as an occasional ingredient. The felspars of the sienitic granites and hornblende rocks are generally of a greyish colour.

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* The above arrangement of the strata has probably determined the direction of the depression marked by the Bunas River in its course through Mewar.

† See p. 264, No. xxviii. of this Journal.

About five miles to the westward of Meirta, we have a narrow bed of a coarse granular crystalline limestone, or rather dolomite, of a smoky white colour, with minute interspersed grains of iron-pyrites *. This is succeeded by a rock composed of quartz, greyish felspar, and hornblende, the last mineral being so disposed as to impart to the mass a distinct gneissy structure. It is a stratified rock, though the stratiform structure is somewhat obscure, and may be termed a sienitic gneiss. We have then a succession of granitic rocks having the general characters above enumerated, and these, about forty miles to the east of Meirta, gradually pass into rocks of the micaceous schist series, of which hornblende schist, chlorite schist, schistose quartz, and granular limestone, are members. These again are succeeded by argillaceous schists and quartz-rocks, which dip under the newer sandstones of the north of Malwa.

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* Marble is of abundant occurrence in Central India, and is extensively quarried for architectural purposes. The principal quarries in this neighbourhood are at Kunkmowllee, a town situated on a lake about thirty miles north of Meirta. The marble of this place is rather of a coarse texture, and is interstratified with micaceous schist passing into gneiss and hornblende rock. The strata are inclined, dip to the east of south, and present other peculiarities which we have no space to enumerate at this time. Another quarry, lately opened, is situated between the forts of Mandalghur and Humeerghur in the east of Méwar. The marble here is of a closer texture than the last, and is of a pure white colour; it occurs interstratified with the rocks of the micaceous series, and is a very beautiful variety. A peculiarly elegant temple belonging to a siek of Jains has lately been constructed of this marble at Shapoora. The quarries from which the Agra and Dehlia districts have been

At and immediately to the west of Meirta, there occurs interstratified with the gneisses and hornblende rocks a series of granular felspars, occasionally containing minute grains of chlorite or scales of mica. This rock is analogous to the *Weiss stein* of Werner; but as its colour is as generally reddish as white, the name is not very appropriate: in its texture it inclines to schistose. It occurs more abundantly as we approach the hill ranges which bound the Valley of Oodipoor, and is interstratified with a translucent quartz-rock, which last is arranged in parallel bars or plates separated from each other by minute scales of mica. These can only be observed when the mass is broken in a direction parallel to the slaty structure, and the quartz plates are frequently so thin that the rock acquires a minutely fibrous texture. Another variety of this rock has a specific gravity much higher than that of common quartz; it is of a light yellowish-green colour, before the blowpipe is converted into a dark

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supplied are at Makrana on the borders of Jondpoor and Ajmeer. These I have not visited. Besides the above, marble is found in many other localities, as for instance near Salúmber in Méwar, at Mungarwar to the west of Neemuch, and near Jeypoor, but is not extensively worked. The yellow-coloured marble which Bishop Heber saw at Agra, and which he terms Sienna marble, is a secondary limestone from Jesselmere. It includes numerous minute fragments of shells, and a particular variety has, at a little distance, exactly the appearance of the moss jaspers of Scotland. This rock Colonel Todd calls "Jasper rock." It seems to belong to a secondary limestone formation extensively distributed to the west of the Aravulli chain, and which extends from Jesselmeer into Guzerat. It is used in Calcutta as a lithographic stone, and contains numerous organic remains of shells which have not as yet been described. Several specimens are in the Bombay museum.

brown scoriæ, and seems rather to be a variety of epidote; it is intimately associated with the quartz-rocks, and is isomorphous with and passes into them. As we proceed west, the quartz-rocks pass into micaceous schists which alternate with hornblende schists. A variety of the last mentioned rock consists of a basis of a dark colour resembling the more compact hornblende schist, or rather greenstones, through which are distributed numerous scales of mica. It forms the link between the micaceous and hornblende schists, and may be termed a micaceous hornblende schist.

We have now approached close upon the base of the boundary ranges, and the rocks have been gradually becoming more and more allied to the rocks of the argillaceous schist series. The strata which constitute the most eastern of the boundary ranges consist of quartz-rock, alternating to the westward with argillaceous schists. A similar quartz-rock also occupies the plain in the immediate neighbourhood of the eastern slope of the barrier range. The first indication of the occurrence of rocks of this nature may be traced in a series of strata observed immediately to the eastward of the quartz-rocks. The strata in this position consist of a light greyish-coloured rock, somewhat friable, and in appearance and texture resembling the finer freestones. It is more or less schistose, can be scratched by the knife, and the finer varieties have a slightly soapy feel; the coarser, on the other hand, are gritty *en masse*, but when reduced to powder have also a soapy feel. This rock seems to consist of quartz, or rather silica, intimately blended with talcose matter, the magnesia of which has probably operated in preventing a more perfect crystallization. It passes insensibly into the quartz-rock of the barrier range, the geology of which we shall now proceed to examine, merely premising, that the approach to this range is marked by the occurrence of several detached ranges and outlines composed of the rocks we have just described.

At the *bund* of the Oodissagor, we have an excellent opportunity of studying the internal structure of the most eastern of the barrier ranges of the Valley of Oodipoor. The range in this position is traversed by a ravine or narrow cross valley which originally gave issue to the Bedus, but which is now obstructed by a strong and well built *bund*, to the westward of which the waters of the river have been accumulated so as to form a picturesque lake of considerable depth and extent. The *bund* rises about thirty or forty feet above the level of the lake, the waters of which are allowed a partial vent through a narrow chasm which traverses the range immediately to the south of the *bund*.

Both the bund and chasm have been described by Captain Dangerfield* with his usual correctness; and I shall therefore only remark on this head, that while Captain Dangerfield attributes the formation of the latter to a violent convulsion of nature, the natives themselves maintain that it is artificial, and that it was effected by order of Ranah Oodising, who, disliking to cut the bund itself for fear of weakening it, took this method of lowering the level of the lake, which encroached too much on the cultivated land of the valley. I confess I see no reason to doubt the truth of the native account, the chasm having every appearance of being artificial.

The chasm in question traverses the range at right angles to the direction of the strata. Its breadth is about fifty feet; its length is nearly 200 yards, and, during the rains, the water rushes through it with great rapidity, boiling and foaming in its course, and terminating in a cascade, which, though only a few feet in height, has still a very imposing effect. The Bedus then flows slowly onward through the plains of Méwar till it joins the Bunas at Chitor. The sides of the chasm are rocky and nearly perpendicular. The range traversed bears N.N.W. and S.S.E. The hills composing it are generally ridge-shaped, the slopes on either side terminating in a sharp spine either denticulated or forming an even continuous line; the strata are parallel with the range, and are nearly vertical or slightly inclined to the E.N.E. Towards the centre of the chasm the cliffs are about 160 feet in height, and, from this point, which corresponds with the spine of the range, they slope on either side. The preponderating rock in this position is quartz, with a texture more or less schistose, and frequently including scales of mica. It is very generally ferruginous, the colour of the different strata varying considerably, and imparting to the sides of the chasm a vertically zoned aspect. The strata vary in breadth, from a few inches to a couple of feet; they are not distorted, but exist in the form of huge rectilinear tables of great extent and continuity, the truncated edges of which give rise to a structure which Captain Dangerfield seems to have confounded with the columnar.

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* See Malcolm's Central India, Appendix, vol. ii.

The quartz-rocks, though they preserve the above general characters, vary considerably in respect of their internal texture and composition. The purest variety is of a bluish colour, is translucent at the edges, and has a uniform compact texture. Another has a reddish tint, is nearly opake, contains a considerable proportion of associated felspar, and has occasionally a fine angulo-granular approaching to granitoidal texture. In some varieties scales of mica are abundant, and in others the mica is replaced by minute grains of a white pulverulent talcose matter. All, however, are remarkable for the extent and continuity of the tabular masses into which they may be split, a property which they possess in common with several other of the schistose rocks in the neighbourhood, and which distinguishes them from a variety of white quartz-rock extensively distributed in Central India, and which is characterized by the numerous joints and fissures which traverse its substance.

Towards the eastern extremity of the chasm there occurs a band, about eighty feet in breadth, of strata, composed of an extremely soft and friable rock of a light greyish colour and silky aspect, occasionally dull and earthy, with interspersed silky scales. This rock crumbles between the fingers into a fine saponaceous powder; it is distinctly schistose, though the schistose texture is often obscured by the pulverulent nature of the rock, and scales of mica of brass-yellow colour are sparingly interspersed through its substance. It may be described as a variety of talcose schist. This band is traversed longitudinally by numerous thin seams of quartz, often less than an inch in breadth. These preserve the same rectilinear course as the strata; they are of uniform thickness, and the quartz composing them, though, generally speaking, similar to that of the range, is occasionally so intermixed with talcose matter, that it assumes a softer and more loosely aggregated character, and exactly resembles the rock described above, as occurring immediately to the eastward. From this circumstance, I was led to infer the composition of the latter, and it affords one of the many proofs of the gradual passage of one series into another. In fact, all the rocks which we have as yet examined, are but compounds of the same chemical ingredients, differing from each other in their external characters according to the combinations which these ingredients have formed amongst themselves. Many causes may have influenced these combinations, and, among others, a disturbance in the balance of affinities, induced by the loss or diminution of any one of the ingredients present. Our approach to any particular set of strata, is marked by a change in the characters of the rocks we are leaving, and either the preponderance of a mineral constituent previously observed, or the partial occurrence of some new combination into which the original chemical ingredients have entered, prepares us for the change about to be observed. In the case before us, an approaching change in the particular condition of the forming cause which gave origin to the quartz-rocks, is indicated by the occurrence of the band of talcose schist, and the effects of this change are observed in the talco-siliceous rocks which succeed. The latter, as we proceed east, gradually assume a more granitoidal aspect. The silica, which was in the first instance distributed generally through the mass, assumes the form of distinct grains or crystals of quartz. With this felspar, also in crystals, is associated; and the mica, chlorite, talc, and other constituents of the micaceous schists which follow, mark but stages in the same series of changes. The connexion between talc and mica is well understood, and the other mineral compounds differ from each other not so much in their chemical composition, as in the proportions which the different ingredients forming them bear to one another.

In the above enumeration, the changes alluded to have been mentioned in the inverse order in which they probably occurred; as we proceed westward, however, we shall perceive a similar succession of changes and gradations in the rocks of the argillaceous schist series, which now come under our consideration.

(To be continued.)

REMARKS ON THE GYPSIES.

There are few questions in the history of the human species more curious than that of the origin and character of this singular people. A race of men which presents the most extraordinary phenomenon in social life, has existed nearly four centuries in Europe; and yet remains but imperfectly known. Neither time, climate, politics, nor example, have produced any change in their institutions, their manners, their language, or their religious ideas. The Israelites are the only people, who have preserved, like them, their primitive character in foreign lands.*

Different writers have assigned to these people a very different origin—one traces them from the eastern part of Tunis—another from Zanguebar—one from mount Caucasus —one considers them as German Jews—and others bring them from Egypt, Colchos, the Ukraine, &c.

We know of about three writers who have placed this question in a true point of view. The two first, whose opinion is admitted by the learned generally, are Grellman and David Richardson, who consider India as the cradle of the Tzengaris or Gypsies; Abbe Dubois places them among the Kouravers of Mohissoun, while others trace them to the country of the Mahrattas as their original position, where, indeed, they are still found united in tribes.

The primitive tribes of the Tzengaris is a subdivision of different tribes of Parias or men out of caste. The origin of Parias is very ancient. This sub-caste is formed by the union of individuals driven from different castes for offences committed against the religion and laws, and includes a great number of tribes, among whom may be reckoned *Vallouvers*, the *Chakalis*, the *Moutchiers*, &c., and lastly the *Tzengaris*, the primitive tribe of our Bohemians and Egyptians, or the Zingari of the nations, which term still resembles the original name.

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* *Names by which they are known in the different countries in which they reside.*—The Arabs and Moors call them *Harami*, (robbers); the Hungarians, *Cinganys*, and *Pharaoh Nepek* (people of Pharaoh;) the latter name is also given them in Transylvania; the English have adopted the name of *Gypsies*, an alteration of the word *Egyptians;* the Scotch, that of *Caird;* the Spanish call them *Gitanos;* the Portuguese, *Ciganos;* the Dutch, *Heidenen*, (idolators); the Russians, *Texengani;* the Italians, *Zingari;* the Swedes, *Spakaring;* the Danish and Norwegians, *Tatars;* the Wallachians, Bessarabians, Moldavians, Servians and Sclavonians, *Cigani;* the Germans, *Zigeuner;* in France they at first received the name of *Egyptians* and more recently that of *Bohemiens*, because the earliest of the tribe came into France from *Bohemia.* Historians of the middle ages designate them by the name of *Axinghans;* the modern Greeks under that of *Atinghans;* in *Adxerbaidjan*, they are called *Hindou Karuch*, (black Hindoos); in Persia, *Louri;* the Bucharians and inhabitants of Turkistan, call them *Tziaghi*, which appears to be the root of *Tchingeni*, the term given by the Turks to this wandering race. I have been acquainted in Europe with three of their *Rabers* or chiefs, who assure me that they call themselves *Roumna Chal.* These two words belong to the Mahratta language and signify men who wander in the plains. I consider *Tzengaris* as their primitive name, and which is still preserved in their mother country.

The tribe of Tzengaris, called also *Vangaris* on the coast of Concan and of Malabar, is nomadic. They are often met in whole bands near the ancient and magnificent city of *Visapour*, and in the vicinity of *Bangalore* and *Mahissour*, which is called *Mysore*, from a habit of disfiguring eastern names. They are in general of a dark complexion, which justifies the Persian appellation of Black Hindoos. Their religion, institutions, manners, and language differ from those of other tribes of Hindoos. During a war they are addicted to pillage, carry provision for the armies, and fill them with spies and dancers. During peace they make coarse stuffs, and deal in rice, butter, salt, and opium, &c. Their women are as handsome and agreeable as the generality of Hindoos, but are very lascivious. They often carry off young girls whom they sell to natives and Europeans. They are accused of immolating human victims to their demons and eating human flesh. They every where follow the trade of errand runners and procurers. The women are fortune-tellers, a business which they practise by striking on a drum in order to invoke the demon; then pronouncing with the air of a sybil, and with rare volubility, a string of mystical words, and after having gazed at the sky and examined the lineaments of the hand of the person who consults them, they gravely predict the good or evil which is to be his destiny. The women also practise tatooing, and the figures of stars, flowers, animals, &c. which they imprint upon the skin by puncturation and vegetable juices, are ineffaceable. They live in families, and it is not rare to see father and daughter, and uncle and niece, brother and sister, living like beasts together. They are suspicious, liars, gamblers, drunkards, cowards, poltroons, and altogether illiterate; they despise religion, and have no other creed than the fear of evil genii and of fatality. They originated in the province of *Mahrat* among the eastern Ghauts.

The celebrated *Cherif Eddin* assures us that Timur sullied his conquests by the massacre of 100,000 prisoners, Persians and Hindoos. The Monguls spread such terror in all parts of India, that great numbers abandoned that unhappy country. The Hindoos of the three first castes, indeed, remained firm to their country;—their religion made it a duty:—but no place could retain the Soudras and Parias. They are such vagabonds that travellers have met with them in Abyssinia, in Arabia, at Tzouakem in the Persian Gulf, at Penang, at Singapore, at Malacca, at Manilla, at Celebes, at Anyer, and even in China.

Is it not natural to believe that the Tzengaris, who are so accustomed to a camp life, and excluded from Hindoo communion, should practise, or feign to practise, religion which offered them so many advantages, that they should act as spies and purveyors to the Mongul armies, and that a portion of them should accompany Timur in his long traverse through Kandahar, Persia and Bukahra; and after passing through the Caspian and Caucasian regions, and leaving behind them a train of detached families, they should have come to a stand, some in Russia, others in Asia Minor; that a second column should have passed from Kandahar into Mekran and Irak-Arabia; and a third strayed into Syria, Palestine, and Arabia-Petrea, and should have reached Egypt by the Isthmus of Suez, and thence should have passed into Mauritania.

It is not improbable that these rude travellers landed from the Black Sea and Asia Minor in Europe, by the intervention of the Turks during their wars with the Greek empire; and it is equally probable that the first of them who came to Europe, sojourned in European Turkey, as Aventine informs us, and proceeded thence to Wallachia and Moldavia. In 1417, they were found in Hungary, and at the conclusion of that year they were seen in Germany and Bohemia; the next year in Switzerland, and in 1422 in Italy. Pasquier carries their origin in France to 1417, and says that they styled themselves Christians from Lower Egypt, expelled thence by the Saracens, but that in reality they came from Bohemia. From France they passed into Spain and Portugal, and afterwards under Henry VIII. into England. Their hordes commonly consist of two or three hundred persons of both sexes.

Although it is difficult to explain how they acquired the name of Gypsies or Egyptians, it is certain they neither have an Egyptian origin, nor came from Egypt to Europe, as Crantz and Munster have proved.

Countries in which the Tzengaris or Gypsies are now found.—These people constitute part of the population of all the countries of Europe and of a large portion of Asia. In Africa they are found only in Egypt, Nubia, Abyssinia, Soudan and Barbary. They have never appeared in America.

They are most numerous in Spain, Ireland, Turkey and Hungary, but especially in Transylvania, Moldavia, Wallachia, Sclavonia, Courland, Lithuania, and the Caucasian provinces.

In England they are still pretty numerous, but are found only in distant places, seldom coming into the towns excepting in small companies of two or three persons. In Germany, Sweden and Denmark, they have become rare, as also in Switzerland and the Low Countries. In Italy their numbers are diminished. In Spain it is said that there are fifty or sixty thousand of them, and in Hungary, according to the best information, about fifty thousand. In Transylvania, they are most numerous, for in a population of 1,720,000 souls there are reckoned 104,000 Tzengaris. We do not exaggerate in estimating the Tzengarian or Gypsy population of Europe at nearly a million: in Africa, at 400,000; in India, at 1,500,000; and about 2,000,000 in all the rest of Asia, for except in Asiatic Russia, China, Siam, Annan and Japan, they are every where to be found. Hence we may deem the total population of these people to be *five millions.*

We have thus a considerable portion of the human race thrown, as it were, beyond the common rights of nations; so many men wandering about without any claims which can attach them to the soil, encamping in places remote from civilization, living by theft and deception, and every where diffused, notwithstanding the persecution and contempt which are heaped upon them.—*G. Louis Domeny De Rienzi.*

ELECTRO-MAGNETIC EXPERIMENTS. *Communicated by the Author.*

(To the Editor of the Edinburgh Philosophical Journal.)

As there can be but few experiments which may not prove interesting to some part of the scientific world, I send you some of the particulars of one in which I have been lately engaged, in case you should think them worthy of a place in your Journal. It was my wish to have a very powerful magazine of the magnetic fluid, as a basis for some experiments intended to develope, if possible, something more of its physical nature. There was an iron bar, of the best Scotch iron, 60 lb. in weight, rounded by the hammer from the square. It was turned into the form of a horse-shoe magnet, the arch at the top being a semicircle, and the sides proceeding from it in right lines, till their extremities approached to 3½ inches distance between their inner limbs. The diameter of the semicircular arch is 14 inches, that of the iron itself 2 inches. On this was coiled copper-wire, of thickness varying from a tenth of an inch, its general diameter, to one-twelfth. There were 36 coils put on, each of such a length that rather more than 900 feet of wire embraced the iron. The coils were put on in three tiers; the first tier enveloped about 14 inches in length of the iron from each extremity,—the second tier covered the interval between the ends of the first and the spring of the arch,—and the third covered the arch. The first tier again consisted of 9 thicknesses of wire, the second of 7, and the third of 4; so that the first contained 18 coils, the second 14, and the third 4. The total length of wire in each coil was 10 yards, and the average length of each embracing the iron 26 feet. To insulate the wire it was not lapped; the operator had no assistance, and that would have been an endless labour: the following method was adopted.

The iron was first lapped with varnished silk: on this was coiled five lengths of naked wire, two forming the first tier, two the second, and the remaining one embracing the arch: thus the iron had one complete envelope of wire, and as the intervals between the turns of the coils averaged one-quarter of an inch, the wire was effectually insulated from contact with itself or with the iron. The copper so put on was then overlaid with a double thickness of varnished ribbon, varnished in every case *after* it was put on; and a second thickness of copper was coiled on, so that the wire fell into the interstices of the former coils. Again, a double fold of varnished silk was wrapped on, and the operation was thus conducted until there were 9 thicknesses of wire on the first portion of the iron, 7 on the second, and 4 on the arch. The wire was coiled on with great tightness, by means of an instrument made for the purpose, similar to that with which sailors sheath their ropes; viz. a block of hard wood, with a longitudinal channel adapted to the curved surface of the iron, and a hole passing through it obliquely, through which the wire went. The silk envelopes and the varnish seemed not only to insulate the coils, but, when the varnish got fully dry, to unite the whole compound covering into one solid mass. As the ends were now to be soldered together, advantage was taken of the opportunity to determine what effect the wire has in developing the magnetic fluid, according to the distance from the extremities at which it is coiled on the iron. The ends of the three tiers were soldered in succession to main conducting wires, each one-quarter of an inch in diameter, and the effect each time was observed by means of a compass needle. There were several galvanic batteries employed, each consisting of a single pair of plates, all of the same dimensions, and charged each time with acid of the same strength. The needle employed was that of a common theodolite, hung on agate, on a circle of about 5 inches diameter, and graduated to single degrees.

In each experiment the electro-magnet was placed with its poles pointing to the zenith, and in the plane of the prism vertical; and the compass being placed with its centre in the same plane, was moved due westward, when it was shifted. One part of sulphuric acid was mixed with 9 of nitric, and 1 part of this nitro-sulphuric acid to 38 of water formed the acid employed.

Experiment 1.—The first tier of coils was soldered into connexion, the centre of the needle was placed at the distance of 4 feet from the centre of the neighbouring pole of the magnet, and, on the battery being charged, the deflection was 14°. The compass centre was then quickly shifted to the distance of 6 feet, and deflection was 7°.

Experiment 2.—The second tier was then soldered in, and on a fresh battery being charged, distance 6 feet, deflection was 8°; which, on shifting the needle centre to 8 feet distance, diminished to 2° 50′.

Experiment 3.—The arch tier was now lastly soldered in, a fresh battery charged, distance 8 feet, and deflection was 3° 45′; which, on increasing the distance to 10 feet, diminished to 1°. This last deflection, though small, was very decided and well defined.

The following experiment was next made, to ascertain different relative effects of different strengths of acid. The electro-magnet and the needle were disposed as before, the constant distance of the needle centre from that of the neighbouring pole was 10 feet, and the strength of the acid charges was varied.

	°	′
1st charge, 1 of acid to 40 of water, deflection . .	2	15
2d do. 1 of acid to 20 of water, do. . .	3	30
3d do. 1 of acid to 10 of water, do. . .	4	12
4th do. 1 of acid to 6 of water, do. . .	4	15
With this last charge still in, the distance was increased		
to 11 feet, when deflection decreased to . .	3	45
——— to 12 feet, do. do. . .	2	
——— to 13 feet, do. do. . .	1	15
——— to 14 feet, do. do. . .	1	

There will be observed a great disproportion between the results of the first of these latter experiments and the last of the former; this the operator can only attribute to the difference between the times at which the two observations were taken; the former having been taken some time after the battery had exerted its full energy, and the latter immediately after the battery was charged, and while it was in its highest state of activity. Lastly, three of the batteries were taken and charged, 1*st*, two together connected as one pair; 2*d*, the three together as one pair; and, 3*d*, the three as a series of three pairs; and it was observed that the effect produced was precisely the same as if only one pair had been employed with a charge of the same strength. Each pair consists, it should have been stated, of one plate of zinc, 1 foot square, opposed by one similar surface of copper.

The operator has not had any means of testing fully the suspension powers of the electro-magnet; of its enormous coercive power, however, he can give a striking example. A steel-bar (of cast-steel), bent into the horse-shoe form, and weighing 32 lb., was passed once over the poles of the electro-magnet, while excited by a battery charged with 1 of acid to 12 of water, and immediately the steel supported itself with ease from a piece of soft iron. In order to render the above experiments more complete, an attempt was made to ascertain the degree in which each successive thickness of wire contributed to the sum of magnetic influence excited by all together; but it was found that the partial contact which the method of construction adopted allows of among the wires of the same tier, before they enter the insulating envelopes, interfered with the experiments; for, when one wire only was introduced into the galvanic circle, it communicated the galvanic fluid to all the other members of the same tier by mere contact. It appears to the operator, that the best way of preparing a bar for an electro-magnet of very large size, would be to have it made up of several small bars, by fagotting and welding. I have the honour to be, Sir, your obedient servant,

φ. μ.

ON THE ORIGIN OF METEORIC STONES. *By F. G. FISCHER, Esq.*

AMONG the multitude of wonderful occurrences which, within no remote period, have been passing in the moral, political, and even in the physical world, meteoric stones unquestionably belong, the descent of which from the atmosphere was formerly comprehended among the number of nursery tales, but the reality of which has been so triumphantly shewn by Chladni.

Since this singular fact has been placed beyond all doubt by frequent and unequivocal observations, the ingenuity of naturalists has been exhausted to explain it. But every explanation which has been attempted, at least such as have come into repute, rest upon assumptions not less extraordinary than the fact to be explained.

Chladni's explanation seems to have come most into vogue, who regards them as insulated masses wandering in free space, which, on accidentally coming within the sphere of the earth's attraction, are born down by the force of gravity. I am not aware that he has any where attempted an explanation of the origin of such masses, of which there must be an immense profusion in the bounds of space; they must, however, be considered either as materials for the construction of new worlds, or as the fragments of shattered systems, unless we attach to them no manner of importance whatever. Besides this obscurity of their origin, their peculiar chemical composition, in all nearly alike, appears to me nearly inexplicable.

The idea of their being projected from volcanoes in the moon, seems to be still more extravagant, although La Place has shewn, by computation, that the requisite velocity at the outset is not altogether inconceivable. The great number of meteoric stones, and those various directions of their motions, which appear to have no relation to the position of the moon, cannot be made intelligible without new and equally extraordinary assumptions.

Some have even conferred upon the North Pole an enormous volcano, hurling its eruptions to the distance of many hundred miles. But, besides the romance of this notion, it has still the mighty difficulty,—meteoric stones in general have no resemblance to the ejected matter of known volcanoes.

The least extraordinary explanation, that meteoric stones are, perhaps, formations of our own atmosphere, has, in our age, credulous of wonders, attracted the least share of atttention. It is certainly worth while, however, to bring light to bear upon the subject from this quarter, for, although this method of explanation has also no small difficulty to encounter, the requisite assumptions are not merely vague speculations, but can all be supported, partly by established laws, and in part, at least, by analogies. Moreover, it does not exclude the possibility of its truth or falsehood being ascertained from future observations and inquiry.

Before, however, entering upon such an explanation, it may be necessary to premise some general observations upon the properties of our atmosphere.

We should unquestionably form a very imperfect representation of its nature, were we to believe that it contained nothing more than what has been discovered by chemical analysis, which gives as essential component parts, oxygen and nitrogen, with an accidental admixture of aqueous vapour, and a small portion of carbonic acid.

When we reflect how many gases and exhalations are continually evolved at the surface of the earth, it cannot be doubted that many matters must exist in the atmosphere which escape chemical investigation, either because we have no tests to denote their presence, or because they are in too small quantity, or, lastly, because they do not accumulate in the lower regions of the atmosphere, where alone experiments can be instituted.

Let us consider, in the first place, how many gaseous substances are produced continually by the combustion of all kinds of inflammable bodies, either by the intervention of man or in operations of nature.

Let us advert farther, to the multitude of vapours from the imperceptible perspiration of all bodies both in the animal and vegetable kingdoms, which pass at every moment into the atmosphere. No one will surely maintain, that these consist of nothing else than aqueous vapour. Even the odour, which most organic exhalations emit, sufficiently shews that they are not pure aqueous vapour. When we reflect, however, that all changes of organic substances are not sudden transformations, but proceed imperceptibly by an infinite variety of steps, perhaps the most just representation would be, that each organic exhalation is a fluid vapour of a peculiar kind, the basis and chief ponderable parts of which consist of aqueous vapour, with which, however, other organic matter exists in intimate union, bearing the character, not so much of a chemical as organic combination. Now, although the existence of such be not indicated by any chemical tests, they render themselves nevertheless perceptible in daily experience. As a particular instance, all rain and snow water is manifestly impregnated with organic matter. When carefully filtrated, the paper must frequently be changed from the pores becoming obstructed,—a proof that some slimy matter is contained in it, and, when frozen in a strong cold, there generally remains in the centre of the mass a small quantity of a yellow viscous fluid uncongealed. The strongest proof of all, however, appears to me to be the fertilizing power of rain. Such exhalations frequently accumulate so as to become a palpable fog, and the odour which this frequently has, shews plainly enough that it is not simply aqueous vapour. It may be allowed us to conjecture, that it is the composition of such organic exhalations which makes the inhalation of the air beneficial or hurtful to health, and productive of prevailing distempers.

But there must necessarily be in the atmosphere, not merely exhalations of organic origin, but also many inorganic matters, whose existence is not even conjectured, because either from their subtilty or small quantity they escape all chemical tests. The following considerations seem to place this in an interesting point of view.

We are acquainted with two different methods of producing a change on bodies,—heat, and chemical mixture. There are many solids that oppose an obstinate resistance to the former; but certainly there are none which must not give way to the second, nor can any be named which in some one chemical mixture shall not assume either the fluid or the aërial form. There is nothing which endures the action of fire so well as pure carbon, and yet it assumes the elastic form in carbonic acid and carbureted hydrogen gas. In like manner, sulphur in sulphuric acid and in sulphuretted hydrogen gas, silica in fluoric acid gas, and so of others. Fewer examples can, indeed, be given of metals; it is, however, known that, in distilling muriatic acid over clay containing iron, at all times a portion of iron is carried over it. The particles of iron are, indeed, in this case, generally regarded as mechanically suspended; but I cannot comprehend why muriatic acid gas, in other respects so powerful, should not be equally efficient in dissolving oxide of iron as the acid in its fluid state. In short, it is to be apprehended, that the solvent powers of the acid gases have not hitherto been sufficiently ascertained: it is more than probable, that they act both on the metals and metallic oxides. Some chemists, indeed, have observed that even hydrogen gas dissolves a portion of iron.

Be that as it may, there are not wanting other indications that even the metals, and that, too, at the usual temperature, can assume the aërial or gaseous form. That metals raised to an extreme degree of heat can be volatilized, is a fact, and shews at least, in general, their susceptibility of volatilization. But that they are also subject to it, however inconsiderable in degree, at the ordinary temperature, their smell appear to me unequivocally to determine; and the green hue assumed by copper in combustion cannot well be explained otherwise than by the diffusion of a fine metallic vapour.

Evaporation may perhaps be assumed as the general law of all bodies whatever. That all fluid bodies, without exception, evaporate, is undoubted; but with respect to solids this is more obscure. However, the same reason applies, as in the case of metals, to all bodies which emit a peculiar odour. I may here also appeal to a common phenomenon, which is indeed usually differently explained, but which may at least deserve a more attentive consideration. Every one knows that in a well closed room, or press, a quantity of dust in time accumulates. Even within a watch, fitted with the utmost nicety, particles of dust shew themselves from time to time, both in the interior and on the dial-plate. Whence is this? It is generally explained, without farther investigation, by the entrance of extremely fine particles by the minutest chinks and apertures; and part of it may in this way be accounted for. But there are other circumstances which point to a different origin. Where dust is collected in quantity, for instance, in a library, there is a peculiar smell perceptible; are we not hence entitled to conclude that the dust is rather a precipitation from the incumbent air, than something conveyed thither by purely mechanical means? When there is another apartment adjoining to one filled with books, equally closely shut, and left undisturbed for an equal period, dust will be found in both, but neither of the same kind, nor having the same smell, as ought to be the case were it merely admitted by the apertures of the doors and windows. A library presents still another argument for this hypothesis, which is, that the paper of the books evidently undergoes, in process of time, a chemical change. It becomes yellow, friable, and lighter. Unquestionably this change arises from certain component parts of the paper becoming volatile, which no doubt occasions the peculiar smell; and why should it be impossible that these, while in suspension, should be subjected to new transformations, and at last be precipitated in the form of fine dust?

If observations of this kind do not establish the capability of solid bodies to become volatile, at the usual temperature, it must at least be conceded that this hypothesis does not belong to that class which are purely the offspring of the fancy; for it is supported on facts alone, although it cannot be deduced from them with all the rigour of logical induction.

If this hypothesis be provisionally adopted, till farther investigation, we are necessarily compelled to form a very different representation of our atmosphere than that generally admitted; for then, not only its apparent elements, but an infinite variety of gases and vapours unceasingly emitted from all fluid and solid bodies, must also be admitted as entering into it.

But where do these exhalations remain? And why is it, that, in that part of the atmosphere submitted to our examination, we find only faint traces of inconsiderable quantities of them? I will first endeavour to answer this last question, and I believe this may be done in a very direct manner, by taking into consideration the extreme rarity of these exhalations. How incomprehensibly fine many of them are, is incontestibly exemplified by quicksilver. That it evaporates at the ordinary temperature of the atmosphere, every one may convince himself by daily observation of the Toricellian vacuum. But how inconceivably minute must this vapour be, since in an open vessel filled with quicksilver, after years, scarcely the slightest diminution of weight can be perceived!

If a vapour be, however, perhaps several thousand times lighter than the atmosphere, it must ascend with the rapidity of lightning immediately on being disengaged. Should such a vapour have only a slight affinity to any of the component parts of atmospheric air, it will either not commingle at all, or very sparingly with it, but continue to ascend till it reach a stratum of air of equal rarity with itself. These vapours would therefore collect in the upper regions of the atmosphere, and rest upon the inferior portions, which they would thus leave comparatively pure. Something similar may be seen in fluids having little affinity to each other, such as water and oils, or water and ether, where the lighter separates of itself from the heavier fluid, and rests above it. All known kinds of air unite, indeed, to form a homogeneous mixture; at the same time these mixtures take place more rapidly with some than with others, and with heavy carbonic acid gas so slowly, that it remains in a stratum under the atmospheric air, and, if not disturbed, is very gradually diffused through it.

What becomes, however, of these vapours and gases which, in the lapse of ages, must have been immensely augmented, unless Nature have the means of again disencumbering the atmosphere? Where the accidental mixtures, whose existence is ascertained, are reserved, may be shewn with perfect certainty in some, or conjectured with probability in others. Aqueous vapour returns either in the form of rain or snow. It is not so evident where the carbonic, sulphuric acid, and hydrogen gases, and organic exhalations, remain, which daily, in large quantity, pass into the atmosphere. But it cannot be doubted that Nature applies these to the support of organic life, since it is an established fact that plants and animals receive part of their nourishment from the air. It has also been remarked above, that rains contain organic matter. But what now becomes of the exhalations from metals, earths, stones, and all solid bodies, which according to our hypothesis, accumulate in the highest regions of the atmosphere? Perhaps falling stars, fire-balls, northern-lights, and meteoric stones, are the means by which Nature either transforms them into her own essence, or returns them directly to the earth.

It appears to me to be very favourable to my hypothesis, that all these appearances universally occur only in the upper, never in the lower regions of the atmosphere, for which no cause could be assigned were both these regions of the same nature.

Do we attempt now to determine how and by what means Nature accomplishes the reduction of these vapours? We enter, it must be allowed, on a dark domain, where we can only venture upon conjectures. Before, however, we can make an attempt to solve this question, it may be proper to obviate one of the chief objections against the atmospheric origin of meteoric stones.

Such objection is taken from the greatness of the space requisite to furnish materials for the formation of a stone of considerable size. The known masses of iron in Siberia and Chili, which have the character of meteoric stones, weigh more than 1000 lb., and one newly discovered in Brazil is estimated at 14,000 lb. Would not, it may be demanded, the formation of such a stone exhaust a space equal to an entire ocean of air, and put the whole atmosphere into commotion?

This difficulty disappears when we begin to reduce the matter to measure and number. The air is, indeed, in small masses, very light; but masses of moderate dimensions are heavier than might at first be supposed. It may be calculated from the very accurate experiments executed by Messrs Biot and Arago, that a cubic Prussian foot of air, at zero of Reaumur, and barometer 28 inches, weighs nearly 2¼ loth* Prussian. Hence it follows, that a single cubic rood† weighs about 148 lb., consequently more than 1⅓ cwt. Did meteoric stones originate in the lower regions of the atmosphere, very moderate space would therefore be sufficient for their formation. The air, however, in the upper regions, where said meteors are generated, is very much rarified; and this also must be taken into calculation. Since the barometer on the most elevated mountains, consequently about the height of a German mile (4⅔ English), sinks to 14 inches, the air has there only half the specific gravity as at the level of the sea. Now, if it be assumed that the density of the air decreases in geometrical progression, it may easily be calculated that, at the height of 10 German miles, it will be a thousand times, and at 20 German miles more than a million times, rarer than at the earth's surface. If, therefore, a cubic rood of air weighs 148 lb. at the surface, at a height of 10 miles (47 English) about 1000 cubic roods, and at a height of 20 miles (93 English) 1,000,000 cubic roods will have an equal weight. These are truly large numbers; we will, however, make them still larger. Our hypothesis authorised us indeed to assume that the atmosphere, at such heights, consists almost entirely of such vapours; we will not, however, take advantage of this, but suppose that it there contains only a small proportion of foreign gases, for example, a *promille*,—then a thousand million cubic roods, only about the eighth part of a cubic German mile (4¾ English), at a height of 20 miles (93 English), would contain 148 lb. of this foreign matter. This space, brought into a spherical form, would have a diameter of 1240 roods, but, as seen from the earth, would only appear under an angle of 3½°, and this space would afford matter sufficient for a meteoric stone of 148 lb. Even a space a hundred times larger, which would contain materials for a meteoric stone of 14,800 lb. would, at the height of 20 German miles, as seen from the earth, only appear under an angle of somewhat more than 16°. This method of calculation gives some sort of conception of the minuteness of such spaces, compared with the immeasurable extent of the atmosphere in these regions.

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* *Loth* equal to half an ounce.

† *Rood* equal to 11 Paris feet, 7 inch. 2 lin.

It is also easy to conceive that even a momentary annihilation of such a body of air, could scarcely put the atmosphere into agitation. It would, indeed, occasion violent commotions in the upper regions, but, on account of the much greater rarity of the air, it is not very probable that any movement could be produced thereby in the lower regions of the atmosphere.

But how, and by what agents, are such vapours induced to return again to the solid form? Since electricity plays such an important part in the atmosphere, and its influence in all meteoric phenomena is either recognised, or, with probability, assumed by philosophers; it is natural to have recourse here, in the first instance, to this great and wonderful agent. It may, perhaps, serve as a recommendation to our hypothesis, that, being received as the true one, it appears to throw light on a very dark subject—the origin and changes of the electricity of the atmosphere. For, if the extremely minute volatile particles of solid bodies ascend without mixture, and with extreme rapidity, on account of their lightness, through the under strata of air, both the most powerful means of exciting electricity—friction of conducting and nonconducting bodies, and contact of heterogeneous substances, are in constant operation during their motion upwards. Hence the change of the electricity of the air. And since a highly rarified air is a good conductor for both kinds of electricity, it appears natural, that, in the superior regions, sometimes the one and sometimes the other may accumulate, till they have acquired sufficient force to produce great effects. These effects may be of very different kinds, either according to the variety of vapours which may have assembled in any particular space, or the different nature of the excited electricity.

Philosophers almost with universal consent hold the Northern Lights for electrical phenomena; our hypothesis would shew that the so-termed artificial *aurora*, is, in reality, similar to the natural, and it appears even to assign the reason why these phenomena chiefly predominate in the polar regions. Since, in the lower strata of air, where our electrical experiments are conducted, only an inconsiderable portion of the exhalations from solid bodies is contained, it is not improbable that these luminous appearances take place only when the electricity is accumulated in a comparatively pure rarified atmosphere, to which, on the contrary, heterogeneous mixtures are unfavourable. Unquestionably, however, the atmosphere in the polar regions is freer from exhalations from solid bodies, than in the temperate and torrid zones; partly because, in the colds of the north, exhalations go on slower, and in less quantity; partly because there the under strata of air are almost exclusively in contact with water and ice; and partly, in fine, because there, there is little organic life, little corruption or combustion. We are, therefore, entitled to assume that, above the Polar zones, the higher regions of the atmosphere are extremely pure, since, during intense degrees of cold, only a very small quantity, even of aqueous vapour, can be generated. Metallic and earthy exhalations appear to exist there only in very small proportion, and it is only, perhaps, the constant tendency of the atmosphere to an equilibrium which conveys any thither.

In many places inflammable gases, perhaps hydrogen, may ascend to the higher regions. Do they find there others with which they have little affinity, little or no union will take place. But, by the movements which indubitably occur in these higher regions, such vapours may be arranged in extended strata, and, when they are inflamed at the one extremity by an accumulated electricity, become falling stars, or, when the accumulation is very great, form larger fire-balls.

Lastly, at times, a great quantity of metallic vapours, or such as are the products of earthy matters, may be here and there collected, and then, by an accumulated electricity, be determined to return to the solid state, which would be the origin of meteoric stones. And we have shewn above that the spaces requisite for the formation of very heavy masses, are indeed great, abstractly considered, but yet form a proportionally small part of the aërial regions, and hence, in the great operations of Nature connected with the atmosphere, do not exceed the limits of probability.

But how comes it that such vapours, on their return to the solid state, do not resolve themselves into an infinitely fine dust, instead of being precipitated in large compact masses? This appears to me to be the natural consequence of the properties of the matters themselves. In solids, the attraction of cohesion is beyond comparison greater than their gravitation; in fluids, on the contrary, it is less in the same proportion. Hence, when a fluid is precipitated, as water in rain, each particle, when formed, will obey the law of gravitation, consequently descend, and, only in the act of falling, be augmented by the running of several into each other. But, if that which is precipitated or formed be of a solid nature, a very large quantity of cohesive power is set free, at the moment of transition, which must necessarily draw together into one mass, all the gaseous matter. To this also is to be added the consideration, that the same matter, which, as an elastic fluid, occupied a space of many thousand cubic roods, on being reduced to the space of a few cubic inches, gives out a great quantity of heat. Hence the solid body cannot fail to be, at the outset, in a high degree of ignition. It passes, therefore, without doubt, in a melted state, though probably only of momentary duration, from the elastic to assume the solid form, by which the consolidation of the mass is rendered still more comprehensible.

In what way electricity operates this reduction I venture not to determine. But it is easy to perceive that the obscurity which here reigns, does not arise from the vagueness of our hypothesis, but from our still more deficient knowledge of electricity. For it is obvious, if we knew with certainty how electricity acts upon every species of matter, in all circumstances, we could at once determine, with confidence, for or against our hypothesis.

It seems to me, however, that all the observations of whatever kind hitherto made on electrical phenomena, fall quite naturally in with our hypothesis, and we proceed, therefore, to examine some of these more in detail.

We remark, then, the similar composition of all meteoric stones, explained by no other hypothesis. According to ours, it necessarily follows that the exhalations which ascend from the earth are always the same, and that only the relative quantities of their admixture are varied by currents in the atmosphere. We hence ascertain also why these atmospherical productions are of quite a different nature, from the concretions found in the interior of the earth.

Perhaps an objection may be brought against our hypothesis, that in all meteoric stones, certain metals (nickel, for example) occur which are not found any where but in small quantity, and, for the most part, at some depth. But why should not Nature be able to form such metals, since we must admit that she possesses power, in many other cases, to produce metals out of substances not metallic?

Farther, fire-balls move with the greatest velocity in all directions, nay, according to Chladni's observations, sometimes even upwards, which certainly it would be difficult to reconcile with an origin distinct from the earth. Since, according to our hypothesis, the reduction of a quantity of vapour takes place with extreme rapidity, or rather momentaneously, while the entire mass aggregates into a very small place, it is to be supposed that the greater part of the particles must be put into very violent motion. In the forming mass, all these movements combine together in a simple impulse, the direction of which may vary indefinitely, according to the position the vapours may have before and during the reduction. For example, should they, before their reduction, have arranged themselves in a vertical column, the ignition may proceed either from above or below, and consequently, the mass may move either perpendicularly upwards or downwards. The same explanation, it is obvious, may apply to all the cases.

In the first number of Gilbert's Annals for 1817, p. 91, there is a paper by Chladni in which this indefatigable inquirer again mentions a number of observations, whence it indubitably appears that fire-balls have often a bounding motion, similar to rockets. This appearance becomes conceivable when we reflect that, in the perfect state of ignition, affecting the whole mass, new decompositions and internal changes may proceed without interruption. If, during the first reduction, (which is not an effect of heat but of electricity, so that the ignition of the meteor is not the *cause* but the *consequence* of its formation), substances have been found internally which are susceptible of being volatilized by heat, explosions may take place, by which either the whole, as is frequently the case, will be dispersed; or, as also happens, the exploding matter issuing from one or more apertures through which it forces itself, will assume the appearance of streams of fire. But such explosions must, at the same time, also change the direction of the motion, and when many succeed each other in a short interval, such a bounding motion may well be supposed to arise. In the April number of Gilbert's Annals, for 1818, page 299, Chladni mentions a fire-ball, accurately observed and described, of 17th July 1771, which, after descending, exploded, and then rose anew.

But the most singular circumstance appears to me to be one which Chladni has published in the Annals, for 1817, No. 1, p. 96, which redounds very much to the honour of his unbiassed love of truth, since it is not very favourable to his hypothesis of the origin of meteoric stones. He remarks, whenever any one has had an apportunity of observing such a phenomenon from its first commencement, it has generally, at the place, been preceded by a very extensive flash of light, of which a striking example is given in a note, p. 97. To reconcile this appearance with his hypothesis, he supposes the mass, in this instance, to have reached the atmosphere in the state of dust, and then first to have been formed into a compact fire-ball, on this body of dust becoming inflamed by its entrance into the atmosphere. I doubt whether this supposition can be maintained; for it appears to me incomprehensible how this cloud of dust should be united into one body, although each particle were melted, since the attraction of cohesion, which alone could bring about such a concentration, only operates when in contact, or at inconceivably short distances, but which, at the smallest finite distance, is incomparably weaker than gravitation. Such a concentration could, therefore, only be affected by some external power acting mechanically to bring the particles into contact. It is not at all intelligible, however, whence such a power can be derived, especially since the glowing heat of the separate particles must necessarily produce a pressure of the air in all directions. On the contrary, this appearance coincides so well with our hypothesis, that it seems as if nature had been observed in the great laboratory of the atmosphere, in the act of forming a fire-ball or meteoric stone. For should one kind of electricity have sufficiently accumulated at any particular place, and vapours at same time be present, susceptible of reduction by electricity, unquestionably the first effect would consist in the electricity being diffused with the rapidity of lightning through the whole, and making it luminous by the commencement of the reduction, whereby a faint but extensive illumination must be produced. Such an extensive illumination having, it may be, a diameter of several degrees, may, as has already been shewn at a height of perhaps more than twenty miles (93 Eng.), contain matter for the greatest meteoric stones. The illumination, however, cannot be of long continuance, for, as soon as the reduction is complete, the attraction of cohesion, on account of the intimate connexion of all parts of the vapour, would then act with its whole force, and speedily collect the whole into a compact body.

I confess, therefore, that my hypothesis of the origin of meteoric stones, notwithstanding many remaining difficulties, appears to me to be more satisfactory than every other, partly because it presupposes nothing miraculous, nor beyond the reach of analogy, partly also because it seems to afford a solution not merely concerning meteoric stones, but every species of highly elevated luminous meteoric phenomena.—*Berlin Memoirs.*

ON THE DEVELOPMENT OF HEAT IN THE FLOWERS OF THE CALADIUM PINNATIFIDUM. *By Dr E. H.* SCHULTZ *of Berlin.*

IN the botanical garden at Berlin there is a very large plant of the *Caladium pinnatifidum*, which yearly during spring and the first half of summer, produces from twenty to thirty and more flowers. The powerful vegetation of this plant induced me, in the year 1826, to make some experiments in order to ascertain the temperature of one of its flowers, on the supposition that here as well as in many species of the genus Arum, an elevation of temperature would be observed. This was confirmed by an observation which shewed that while the hot-house was at a temperature of 15° R. (61°.1 Fahr.), I saw the thermometer rise from four to five degrees when placed in connexion with the flower. The observation made at that time, I took occasion to insert in the 2d Volume of the work "Die Natur der lebendigen Pflanze," p. 224. To the above I added only that which agreed with other observations, that the part of the calyx, upon which the stamina are placed, became the warmest; that also the whole of the interior of the flower became warm; and that here likewise the temperature again decreased as the flower decayed. M. J. N. Link, who at a later period renewed the investigation upon the same plant, could observe no elevation of temperature; and subsequently Professor Goppert of Breslau thought himself entitled to consider the said elevation of temperature as doubtful.

I had an opportunity this year, during the flowering period of the Caladium pinnatifidum, of observing the above phenomenon anew, and consider it of sufficient importance to be published.

The flowers of the Caladium pinnatifidum quickly decay, *i. e.* in the space of about twelve hours; so speedily, indeed, that the culminating point in the development of the flower is between eight and ten o'clock in the evening. The flowers which open towards mid-day, on the morning of the following day appear completely decayed, and again inclosed in the colourless spathe. During this period of the flower the elevation of temperature falls, and at every other time of the day, the flowers show only the temperature of the surrounding atmosphere; so that Link, not having been attentive to this circumstance, did not find the above elevation of temperature, having measured the temperature of the flower only at mid-day. Thus far I remember, in the year 1826, I measured the temperature between six and seven o'clock in the evening, without, however, being particularly attentive to the period of the flower.

On the 1st of May this year, at mid-day, I had one of the flowers, which was beginning to burst, cut off from the plant, and found before and immediately after the separation, that the temperature of the flower was completely the same with the temperature of the air in the hot-house. I took the flower home with me, and placed it with the cut end of the stalk in a glass of water, in order to observe the phenomena during the period of its blooming. The temperature of the room was 13° R. (61°.2 Fahr.), and the flower had likewise the same temperature until about five o'clock in the afternoon. About six o'clock, the flower, which had been previously without any smell, gave out a very powerful odour that reminded me of trying the temperature. This had risen 2°; for that of the flower was 15° R. (65°.1 Fahr.) At seven o'clock the temperature had risen to 17° (70°.2 Fahr.) At eight o'clock to 19° (74°.7 Fahr.); half-past eight, 19½° (76° Fahr.) At nine o'clock 20½ (78° Fahr.) At ten o'clock 21½ °(81° Fahr.); and this appeared to be the greatest height, since there seemed to be no farther increase up to eleven o'clock. During this elevation of temperature, the disengagement of the odour likewise increased; this became so powerful that the whole room was impregnated with an ammoniacal vapour. In the morning the temperature of the flower had again fallen to the temperature of the air. During the following evening no further elevation of temperature was manifested in this same flower. But, on the contrary, with other flowers which bloomed at a later period upon the same plant, there was the same gradual elevation of temperature and disengagement of odour, which afterwards, during the evening and night, impregnated almost the whole of the air in the hot-house.

When this observation is compared with that of Lamarck and Senebier upon the Arum Italicum, and of Huber upon the Arum cordifolium, there appears a most striking difference in the period of the disengagement of the caloric; likewise in the quantity of caloric, which in the case of the Arum cordifolium rose from 19° to 44°, therefore 25°. Senebier found in Geneva the highest temperature in the Arum Italicum to be developed at a quarter before seven in the evening. On the contrary, according to Bory de St Vincent, the greatest elevation of temperature of the Arum cordifolium in Madasgacar shows itself in the morning after sunrise about seven o'clock; and from eight o'clock the temperature gradually declines until the flower decays: so that in the evening the temperature of the flower differs but little from that of the atmosphere.

The Caladium pinnatifidum, which in Berlin shows the highest temperature of its flower about ten o'clock in the evening, grows wild in the shady forests of Caraccas. Hence it is possible that the periods of its disengagement of caloric, as well as the periods of its blooming, generally regulate themselves according to the peculiarity of the vegetation of the plant, as well as according to the climate of its native country.

ADDRESS DELIVERED IN THE SENATE-HOUSE AT CAMBRIDGE, JUNE 25. 1833, ON THE OCCASION OF THE OPENING OF THE THIRD GENERAL MEETING OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. *By the Rev. W. WHEWELL, M. A. Fellow and Tutor of Trinity College, and one of the Secretaries of the Association.*

THE British Association for the Advancement of Science, meets at present under different circumstances from those which accompanied its former meetings. The publication of the volume containing the Reports applied for by the meeting at York, in 1831, and read before the meeting at Oxford last year, must affect its proceedings during our sittings on the present occasion; and thus we are now to look for the operation of one part of the machinery which its founders have endeavoured to put in action. Entertaining the views which suggested to them the scheme and plan of the Association, they must needs hope that such an event as this publication will exercise a beneficial influence upon its future career.

This hope is derived, they trust, from no visionary or presumptuous notions of what institutions and associations can effect. Let none suppose that we ascribe to assembled numbers and conjoined labours extravagant powers and privileges in the promotion of science;—that we believe in the omnipotence of a parliament of the scientific world. We know that the progress of discovery can no more be suddenly accelerated by a word of command uttered by a multitude, than by a single voice. There is, as was long ago said, no royal road to knowledge—no possibility of shortening the way, because he who wishes to travel along it is the most powerful *one;* and just as little is there any mode of making it shorter, because they who press forward are *many.* We must all start from our actual position, and we cannot accelerate our advance by any method of giving to each man his mile of the march. Yet something we may do: we may take care that those who come ready and willing for the road, shall start from the proper point and in the proper direction;—shall not scramble over broken ground, when there is a causeway parallel to their path, nor set off confidently from an advanced point when the first steps of the road are still doubtful;—shall not waste their powers in struggling forwards where movement is not progress, and shall have pointed out to them all glimmerings of light, through the dense and deep screen which divides us from the next bright region of philosophical truth. We cannot create, we cannot even direct the powers of discovery, but we may perhaps aid them to direct themselves; we may perhaps enable them to feel how many of us are ready to admire their success; and willing, so far as it is possible for intellects of a common pitch, to minister to their exertions.

It was conceived that an exposition of the recent progress, the present condition, the most pressing requirements of the principal branches of science at the present moment, might answer some of the purposes I have attempted to describe. Several such expositions have accordingly been presented to the Association by persons selected for the task, most of them eminent for their own contributions to the department which they had to review; and these are now accessible to members of the Association and to the public. It appears to be suitable to the design of this Body, and likely to further its aims, that some one should endeavour to point out the bearing which the statements thus brought before it may and ought to have upon its future proceedings, and especially upon the labours of the meeting now begun. I am well persuaded that if the President had taken this office upon himself, the striking and important views which it may naturally suggest would have been presented in a manner worthy of the occasion: he has been influenced by various causes to wish to devolve it upon me, and I have considered that I should show my respect for the Association better by attempting the task, however imperfectly, than by pleading my inferior fitness for it.

The particular questions which require consideration, and the researches which most require prosecution, in the sciences to which the Reports now before you refer, will be offered to the notice of the Sections of the Association which the subjects respectively concern, at their separate sittings. It is conceived that the most obvious and promising chance of removing deficiencies and solving difficulties in each subject, is to be found in drawing to them the notice of persons who have paid a continued and especial attention to the subject. The consideration of these points will therefore properly form a part of the business of the Sectional Meetings; and all members of the Association, according to their own peculiar pursuits and means, will thus have the opportunity of supplying any wanting knowledge, and of throwing light upon any existing perplexity.

But besides this special examination of the suggestions which your Reports contain, there are some more general reflections to which they naturally give rise, which may perhaps be properly brought upon this first General Assembly of the present meeting; and which, if they are well founded, may preside over and influence the aims and exertions of many of us, both during our present discussions and in our future attempts to further the ends of science.

Astronomy.—There is here neither time nor occasion for any but the most rapid survey of the subjects to which your reports refer, in the point of view in which the Reports place them before you. *Astronomy*, which stands first on the list, is not only the queen of sciences, but in a stricter sense of the term, the only perfect science;—the only branch of human knowledge in which particulars are completely subjugated to generals, effects to causes; in which the long observation of the past has been, by human reason, twined into a chain which binds in its links the remotest events of the future;—in which we are able fully and clearly to interpret Nature's oracles, so that by that which we have tried we receive a prophecy of that which is untried. The rules of all our leading facts have been made out by observations of which the science began with the earliest dawn of history; the grand law of causation by which they are all bound together has been enunciated for 150 years; and we have in this case an example of a science in that elevated state of flourishing maturity, in which all that remains is to determine with the extreme of accuracy the consequences of its rules by the profoundest combinations of mathematics, the magnitude of its data by the minutest scrupulousness of observation; in which, further, its claims are so fully acknowledged, that the public wealth of every nation pretending to civilization, the most consummate productions of labour and skill, and the loftiest and most powerful intellects which appear among men, are gladly and emulously assigned to the task of adding to its completeness. In this condition of the science it will readily be understood that Professor Airy, your Reporter upon it, has had to mark his desiderata, in no cases but those where some further development of calculation, some further delicacy of observation, some further accumulation of exact facts, are requisite; though in every branch of the subject the labour of calculation, the delicacy of observation, and the accumulation of exact facts, have already gone so far, that the mere statement of what has been done can hardly be made credible or conceivable to a person unfamiliar with the study.

One article indeed in his list of recommendations to future labourers read at the last Meeting of the Association, may appear capable of being accomplished by more limited labour than the rest:—the determination of the mass of Jupiter by observations of the elongations of his satellites. And undoubtedly, many persons were surprised when they found that on this, so obvious a subject of interest, no measures had been obtained since those which Pound took at the request of Newton. Yet on this case, if an accuracy and certainty worthy of the present condition of Astronomy were to be aimed at, the requisite observations could not be few nor the calculation easy, when it is considered in how complex a manner the satellites disturb each other's motions. But the Meeting will learn with pleasure that the task which he thus pointed out to others, he has himself in the intervening time executed in the most complete manner. He has weighed the mass of Jupiter in the way he thus recommended; and it may shew the wonderful perfection of such astronomical measures to state, that he has proved with certainty, that this mass is more than 322 and less than 323 times the mass of the terrestrial globe on which we stand.

Such is Astronomy; but in proceeding to other sciences, our condition and our task are of a far different kind. Instead of developing our theories, we have to establish them; instead of determining our data and rules with the last accuracy, we have to obtain first approximations to them. This indeed, may be asserted of the next subject on the list, though that is, in its principles, a branch of Physical Astronomy; for that alone of all the branches of Physical Astronomy had been almost or altogether neglected by men of science. I speak of the science of the *Tides*. Mr Lubbock terminated his Report on this subject, by lamenting in Laplace's words this unmerited neglect. He himself in England, and Laplace in France, were indeed the only mathematicians who had applied themselves to do some portion of what was to be done with respect to this subject. Since our Meeting last year, Mr Dessiou has, under Mr Lubbock's direction, compared the tides of London, Sheerness, Portsmouth, Plymouth, Brest, and St Helena; and the comparison has brought to light very remarkable agreements in the law which regulates the time of high water, agreements both with each other and with theory; and has at the same time brought into view some anomalies which will give a strong impulse to the curiosity with which we shall examine the records of future observations at some of these places and at many others. I may perhaps here take the liberty of mentioning my own attempts since our last Meeting, to contribute something bearing on this department. It appeared to me that our knowledge of one particular branch of this subject, the motion of the tide-wave in all parts of the ocean, was in such a condition, that by collecting and arranging our existing materials, we should probably be enabled to procure abundant and valuable additions to them. This, therefore, I attempted to do; and I have embodied the result of this attempt in an "Essay towards a First Approximation to a Map of Cotidal Lines," which is now just printed in the Transactions of the Royal Society of London. If the time of the Meeting allows, I would willingly place before you the views at which we have now arrived, and the direction of our labours which these suggest.

In the case of the science of tides, we have no doubt about the general theory to which the phenomena are to be referred, the law of universal gravitation; though we still desiderate a clear application of the theory of the details.

Light.—In another subject which comes under our review, the science of Light, the prominent point of interest is the selection of the general theory. Sir David Brewster, the author of our Report on this subject, has spoken of "the two rival theories of light," which are, as you are aware, that which makes light to consist in material particles emitted by a luminous body, and that which makes it to consist in undulations propagated through a stationary ether. The rivalry of these theories, so far as they can now be said to be rivals, has been by no means barren of interest and instruction during the year which is just elapsed. The discussions on the undulatory theory in our scientific journals have been animated, and cannot, I think, be considered as having left the subject where they found it. The claims of the undulatory theory, it will be recollected, do not depend only on its explaining the facts which it was originally intended to explain; but on this,—that the suppositions adopted in order to account for one set of facts, fall in most wonderfully with the suppositions requisite to explain a class of facts entirely different; in the same manner as in the doctrine of gravitation, the law of force which is derived from the revolutions of the planets in their orbits, accounts for the apparently remote facts of the precession of the equinoxes and the tides. To all this there is nothing corresponding in the history of the theory of emission; and no one, I think, well acquainted with the subject, would now assert that if this latter theory had been as much cultivated as the other, it might have had a similar brilliant fortune in these respects.

But if the undulatory theory be true, there must be solutions to all the apparent difficulties and contradictions which may occur in particular cases: and moreover, the doctrine will probably gain general acceptance, in proportion as these solutions are propounded and understood, and as prophecies of untried results are delivered and fulfilled. In the way of such prophecies few things have been more remarkable than the prediction, that under particular circumstances a ray of light must be refracted into a conical pencil, deduced from the theory by Professor Hamilton of Dublin, and afterwards verified experimentally by Professor Lloyd. In the way of special difficulties, Mr Potter proposed an ingenious experiment which appeared to him inconsistent with the theory. Professor Airy, from a mathematical examination of this case, asserted that the facts, which are indeed difficult to observe, must be somewhat different from what they appeared to Mr Potter; and having myself been present at Professor Airy's experiments, I can venture to say, that the appearances agree exactly with the results which he has deduced from the theory. Another gentleman, Mr Barton, proposed other difficulties founded upon calculation of certain experiments of Biot and Newton; and Professor Powell of Oxford has pointed out that the data so referred to cannot safely be made the basis of such calculations, for mathematical reasons. There is indeed here also one question of fact concerning an experiment stated in Newton's Optics: in a part of the image of an aperture where Newton's statement places a dark line, in which Mr Barton has followed him, Professors Airy, Powell, and others have been able to see only a bright space, as the theory would require. Probably the experiments giving the two different results have not been made under precisely the same circumstances; and the admirers of Newton are the persons who will least of all consider his immoveable fame as exposed to any shock by these discussions.

Perhaps, while the undulationist will conceive that his opinions have gained no small accession of evidence by this exemplification of what they will account for, those who think the advocates of the theory have advanced its claims too far, will be in some degree conciliated by having a distinct acknowledgment, as during these discussions they have had, of what it does not pretend to explain. The whole doctrine of the absorption of light is at present out of the pale of its calculations; and if the theory is ever extended to these phenomena, it must be by supplementary suppositions concerning the ether and its undulations, of which we have at present not the slightest conception.

Heat.—There are various of the Physical subjects to which your Reports refer, which it is less necessary to notice in a general sketch like the present. The recent discoveries in Thermo-electricity, of which Professor Cumming has presented you with a review, and the investigations concerning Radiant Heat which have been arranged and stated by Professor Powell, are subjects of great interest and promise; and they are gradually advancing, by the accumulation of facts bound together by subordinate rules, into that condition in which we may hope to see them subjugated to general and philosophical theories. But with regard to this prospect, the subjects I have mentioned are only the fragments of sciences, on which we cannot hope to theorize successfully except by considering them with reference to their wholes;—Thermo-electricity with reference to the whole doctrine of electricity; Radiant Heat with reference to the whole doctrine of heat.

Meteorology.—If the subjects just mentioned be but parts of sciences, there is another on which you have a Report before you, which, though treated as one science, is in reality a collection of several sciences, each of great extent. I speak of Meteorology, which is reported on by Professor Forbes. There is, perhaps, no portion of human knowledge more capable of being advanced by our conjoined exertions than this: some of the requisite observations demand practice and skill; but others are easily made, when the observer is imbued with sound elementary notions; and in all departments of the subject little can be done without a great accumulation of facts and a patient inquiry after their rules. Some such contributions we may look for at our present Meeting. Professor Forbes has spoken of the possibility of constructing maps of the sky, by which we may trace the daily and hourly condition of the atmosphere over large tracts of earth. If indeed we could make a stratigraphical analysis of the aërial shell of the earth, as the geologist has done of its solid crust, this would be a vast step for meteorology. This, however, must needs be a difficult task: in addition to the complexity of these superincumbent masses, time enters here as a new element of variety: the strata of thc geologist continue fixed and permanent; those of the meteorologist change from one moment to another. Another difficulty is this; that while we want to determine what takes place in the whole depth of the aërial ocean, our observations are necessarily made almost solely at its bottom. Our access to the heights of the atmosphere is more limited, in comparison with what we wish to observe, than our access to the depths of the earth.

Geology.—Geology, indeed, is a most signal and animating instance of what may be effected by continued labours governed by common views. Mr Coneybeare's Report upon this science gives you a view of what has been done in it during the last twenty years; and his Section of Europe from the North of Scotland to the Adriatic, which is annexed to the Report, conveys the general views with regard to the structure of Central Europe, at which geologists have now arrived. To point out any more recent additions to its progress or its prospects is an undertaking more suitable to the geologists by profession, than to the present sketch. And all who take an interest in the subject will rejoice that the constitution and practice of the Geological Society very happily provide, by the annual addresses of its presidents, against any arrear in the incorporation of fresh acquisitions with its accumulated treasures.

Mineralogy.—The science of Mineralogy, on which I had the honour of offering a Report to the Association, was formerly looked upon as a subordinate portion of Geology. It may, however, now be most usefully considered as a science co-ordinate and closely allied with Chemistry, and the most important questions for examination in the one science belong almost equally to the other. Mr Johnston, in his Report on Chemical Science, has, as the subject required, dwelt upon the questions of isomorphism and plesimorphism, which I had noticed as of great importance to Mineralogy. Dr Turner and Professor Miller, who at the last meeting undertook to inquire into this subject, have examined a number of cases, and obtained some valuable facts;

but the progress of our knowledge here necessarily requires time, since the most delicate chemical analysis and the exact measurement of thirty or forty crystals are wanted for the satisfactory establishment of the properties of each species*. In Chemistry, besides the great object of isomorphism to which I have referred, there are some other yet undecided questions, as, for instance, those concerning the existence and relations of the sulpho-salts and chloro-salts; and these are not small points, for they affect the whole aspect of chemical theory, and thus shew us how erroneously we should judge, if we were to consider this science as otherwise than in its infancy.

In every science, Notation and Nomenclature are questions subordinate to calculation and theory. The Notation of Crystallography is such as to answer the purposes of calculation, whether we take that of Mohs, Weiss, or Nauman. It appears very desirable that the Notation of Chemistry also should be so constructed as to answer the same purpose. Dr Turner in the last edition of his Chemistry, and Mr Johnston in his Report, have used a notation which has this advantage, which that commonly employed by the continental chemists does not possess.

I have elsewhere stated to the Association how little hope there appears at present to be of purifying and systematizing our mineralogical nomenclature. The changes of theory in chemistry to which I have already referred, must necessarily superinduce a change of its nomenclature, in the same manner in which the existing nomenclature was introduced by the prevalent theory; and the new views have in fact been connected with such a change by those who have propounded them. It will be for the Chemical Section of the Association to consider how far these questions of Nomenclature and Notation can be discussed with advantage at the present meeting.

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* Perhaps I shall not have a more favourable occasion than the present of correcting a statement in my Report, which is not perfectly accurate, on a point which has been a subject of controversy between Sir D. Brewster and Mr Brooke. I have noticed (p. 338.) the sulphato-tri-carbonate of lead of Mr Brooke, as a mineral which at first appeared to contradict Sir D. Brewster's general law of the connexion of crystalline form with optical structure, inasmuch as it appeared to be of the rhombohedral system, and was found to have two axes of double refraction; and which was afterwards found to confirm the law, the apparently rhombohedral forms being found by Mr Haidinger to be not simple but compound. It seems, however, that the solution of the difficulty (for no one now will doubt that it has a solution) is somewhat different. There appear to have been included under this name two different kinds of crystals belonging to different systems of crystallization. Some which Mr Brooke found to be rhombohedral, Sir D. Brewster found to have a single optical axis with no trace of composition: others were prismatic with two axes; and thus Mr Brooke's original determinations were probably correct. The high reputation of the parties in this controversy does not need this explanation, but probably those who look with pleasure at the manner in which the apparent exceptions to laws of nature gradually disappear, may not think a moment or two lost in placing the matter on its proper footing.

Physiology.—The Reports presented at the last meeting had a reference, for the most part, to physical rather than physiological science. The latter department of human knowledge will be more prominently the subject of some of the Reports which are to come before us on the present occasion. There is, however, one of last year's Reports which refers to one of the widest questions of Physiology; that of Dr Prichard on the History of the Human Species, and its subdivision into races. The other lines of research which tend in the same direction will probably be brought before the Association in successive years, and thus give us a view of the extent of knowledge which is accessible to us on this subject.

The value of Theory in Science estimated.—In addition to these particular notices of the aspect under which various sciences present themselves to us as resulting from the Reports of last year, there is a reflection which may I think be collected from the general consideration of these sciences, and which is important to us, since it bears upon the manner in which science is to be promoted by combined labour, such as that which it is a main object of this Association to stimulate and organize. The reflection to which I refer is this;—that a combination of theory with facts, of general views with experimental industry, is requisite, even in subordinate contributors to science. It has of late been common to assert that *facts* alone are valuable in science; that theory, so far as it is valuable, is contained in the facts; and, in so far as not contained in the facts, can merely mislead and preoccupy men. But this antithesis between theory and facts has probably in its turn contributed to delude and perplex: to make men's observations and speculations useless and fruitless. For it is only through some view or other of the *connexion* and *relation* of facts, that we know what circumstances we ought to notice and record; and every labourer in the field of science, however humble, must direct his labours by some theoretical views, original or adopted. Or if the word *theory* be unconquerably obnoxious, as to some it appears to be, it will probably still be conceded, that it is the rules of facts, as well as facts themselves, with which it is our business to acquaint ourselves. That the recollection of this may not be useless, we may collect from the contrast which Professor Airy in his Report has drawn between the astronomers of our own and of other countries. "In England," he says, (p. 184), "an observer conceives he has done every thing when he has made an observation." "In foreign observations," he adds, "the exhibition of the results, and the comparison of the results with theory, are considered as deserving more of an astronomer's attention, and demanding greater exertion of his intellect than the mere observation of a body on the wire of a telescope." We may indeed perceive in some measure the reason which has led to the neglect of theory with us. For a long period astronomical theory was greatly a-head of observation, and this deficiency was mainly supplied by the perseverance and accuracy of English observers. It was natural that the value and reputation which our observations thus acquired for the time, should lead us to think too disrespectfully, in comparison, of the other departments of the science. Nor is the lesson thus taught us confined to astronomy: for, though we may not be able in other respects to compare our facts with the results of a vast and yet certain theory, we ought never to forget that facts can only become portions of knowledge as they become classed and connected: that they can only constitute truth when they are included in general propositions. Without some attention to this consideration, we may notice daily the changes of the winds and skies, and make a journal of the weather, which shall have no more value than a journal of our dreams would have: but if we can once obtain fixed measures of what we notice, and connect our measures by probable or certain rules, it is no longer a vacant employment to gaze at the clouds, or an unprofitable stringing together of expletives to remark on the weather; the caprices of the atmosphere become steady dispositions, and we are on the road to meteorological science.

It may be added—as a further reason why no observer should be content without arranging his observations, in whatever part of Physics, and without *endeavouring* at least to classify and connect them—that when this is not done at first it will most likely never be done. The circumstances of the observation can hardly ever be properly understood or interpreted by others; the suggestions which the observations themselves supply, for change of plan or details, cannot in any other way be properly appreciated and acted on. And even the mere multitude of analyzed observations may drive future students of the subject into a despair of rendering them useful. Among the other desiderata in Astronomy which Professor Airy mentions, he observes "Bradley's observations of stars" made in 1750 "were nearly useless till Bessel undertook to reduce them in 1818." "In like manner, Bradley's and Maskelyne's observations of the Sun are still nearly useless," and they and many more must continue so till they are reduced. This could not have happened if they had been reduced and compared with theory at the time; and it cannot but grieve us to see so much skill, labour and zeal thus wasted. The perpetual reference or attempt to refer observations, however numerous, to the most probable known rules can alone obviate similar evils.

It may appear to many, that by thus recommending theory, we incur the danger of encouraging theoretical speculations to the detriment of observation. To do this would be indeed to render an ill service to science: but we conceive that our purpose cannot so far be misunderstood. Without here attempting any nice or technical distinctions between theory and hypothesis, it may be sufficient to observe that *all deductions from theory for any other purpose than that of comparison with observation*, are frivolous and useless exercises of ingenuity, so far as the interest of physical science are concerned. Speculators, if of active and inventive minds, *will* form theories whether we wish it or no. These theories may be useful or may be otherwise—we have examples of both results. If the theories merely stimulate the examination of facts, and are modified as and when the facts suggest modification, they may be erroneous, but they will still be beneficial:—they may die, but they will not have lived in vain. If, on the other hand, our theory be supposed to have a truth of a superior kind to the facts; to be certain independently of its exemplification in particular cases:—if, when exceptions to our propositions occur, instead of modifying the theory, we explain away the facts;—our theory then becomes our tyrant; and all who work under its bidding do the work of slaves, they themselves deriving no benefit from the result of their labours. For the sake of example we may point out the Geological Society as a body which, labouring in the former spirit, has enobled and enriched itself by its exertions: if any body of men should employ themselves in the way last described, they must soon expend the small stock of *à priori* plausibility with which they must of course begin the world.

To exemplify the distinction for a moment longer, let it be recollected that we have at the present time two rival theories of the history of the Earth which prevail in the minds of geologists:—one which asserts that the changes of which we trace the evidence in the Earth's materials have been produced by causes such as are still acting at the surface:—another which considers that the elevation of mountain chains and the transition from the organized world of one formation to that of the next, have been produced by events which, compared with the present course of things, may be called catastrophes and convulsions. Who does not see that all that those theories have hitherto done, has been, to lead geologists to study more exactly the laws of permanence and of change in the existing organic and inorganic world, on the one hand; and, on the other, the relations of mountain chains to each other, and to the phenomena which their strata present? And who doubts, that, as the amount of the full evidence may finally be (which may indeed perhaps require many generations to accumulate) geologists will give their assent to the one or the other of these views, or to some intermediate opinion to which both may gradually converge?

On the other hand—to take an example from a science with which I have had a professional concern—the theory that crystalline bodies are composed of ultimate molecules, which have a definite and constant geometrical form, may properly and philosophically be adopted, so far as we can, by means of it, reduce to rules the actually occurring secondary faces of such substances. But if we assume the doctrine of this composition, and then form imaginary arrangements of these atoms, and enunciate these as explanations of dimorphism, or plesiomorphism, or any other apparent exception to the general principle, we proceed, as appears to me, unphilosophically. Let us collect and classify the facts of dimorphism and plesiomorphism, and see what rules they follow, and we may then hope to discern whether our atomic theory of crystalline molecules is tenable, and what modifications of it these cases, uncontemplated in its original formation, now demand.

Morals of Science.—I will not now attempt to draw forth other lessons which the Report of last year may supply for our future guidance; although such offer themselves, and will undoubtedly affect the spirit of our proceedings during this Meeting. But there is a reflection belonging to what I may call the morals of science, which seems to me to lie on the face of this Report, and which I cannot prevail upon myself to pass over. In looking steadily at the past history and present state of physical knowledge, we cannot, I think, avoid being struck with this thought,—How little is done and how much remains to do;—and again, notwithstanding this, how much we owe to the great philosophers who have preceded us. It is sometimes advanced as a charge against the studies of modern science, that they give men an overweening opinion of their own acquirements—of the superiority of the present generation,—and of the intellectual power and progress of man:—that they make men confident and contemptuous, vain and proud. That they *never* do this, would be much to say of these or of any other studies; but, assuredly, *those* must read the history of science with strange prepossessions who find in it an aliment for such feelings. What is the picture which we have had presented to us? Among all the attempts of man to systematize and complete his knowledge, there is one science, Astronomy, in which he may be considered to have been successful; he has there attained a general and certain theory: for this success, the labour of the most highly gifted portion of the species for 5000 years has been requisite. There is another science, Optics, in which we are, perhaps, in the act of obtaining the same success, with regard to a part of the phenomena. But all the rest of the prospect is comparatively darkness and chaos; limited rules, imperfectly known, imperfectly verified, connected by no known cause, are all that we can discern. Even in those sciences which are considered as having been more successful, as Chemistry, every few years changes the aspect under which the theory presents the facts to our minds, while no theory, as yet, has advanced beyond the mere hornbook of calculation. What is there here of which man can be proud, or from which he can find reason to be presumptuous? And even if the Discoverers to whom these sciences owe such progress as they have made—the great men of the present and the past—if THEY might be elate and confident in the exercise of their intellectual powers, who are *we*, that we should ape their mental attitudes?—we, who can but with pain and effort keep a firm hold of the views which they have disclosed? But it has not been so; they, the really great in the world of intellect, have never had their characters marked with admiration of themselves and contempt of others. Their genuine nobility has ever been superior to those ignoble and low-born tempers. Their views of their own powers and achievements have been sober and modest, because they have ever felt how near their predecessors had advanced to what they had done, and what patience and labour their own small progress had cost. Knowledge, like wealth, is not likely to make us proud or vain, except when it comes suddenly and unearnt; and in such a case, it is little to be hoped that we shall use well, or increase, our ill-understood possession.

Perhaps some of the appearance of over-weening estimation of ourselves and our generation which has been charged against science, has arisen from the natural exultation which men feel at witnessing the successes of art. I need not here dwell upon the distinction of science and art;—of knowledge, and the application of knowledge to the uses of life;—of theory and practice. In the success of the mechanical arts there is much that we look at with an admiration mingled with some feeling of triumph; and this feeling is here natural and blameless. For what is all such art but a struggle;—a perpetual conflict with the inertness of matter and its unfitness for our purposes? And when, in this conflict, we gain some point, it is impossible we should not feel some of the exultation of victory. In all stages of civilization this temper prevails: from the naked inhabitant of the islands of the ocean, who by means of a piece of board glides through the furious and apparently deadly line of breakers, to the traveller who starts along a rail-road with a rapidity that dazzles the eye; this triumphant joy in successful art is universally felt. But we shall have no difficulty in distinguishing this feeling, from the calm pleasure which we receive from the contemplation of truth. And when we consider how small an advance of speculative science is implied in each successful step of art, we shall be in no danger of imbibing, from the mere high spirits produced by difficulty overcome, any extravagant estimate of what man has done or can do, any perverse conception of the true scale of his aims and hopes.

Still, it would little become us here to be unjust to practical science. Practice has always been the origin and stimulus of theory: Art has ever been the mother of Science—the comely and busy mother of a daughter of a far loftier and serener beauty. And so it is likely still to be: there are no subjects in which we may look more hopefully to an advance in sound theoretical views, than those in which the demands of practice make men willing to experiment on an expensive scale, with keenness and perseverance; and reward every addition of our knowledge with an addition to our power. And even they—for undoubtedly there are many such—who require no such bribe as an inducement to their own exertions, may still be glad that such a fund should exist, as a means of engaging and recompensing subordinate labourers.

I will not detain you longer by endeavouring to follow more into detail the application of these observations to the proceedings of the General and Sectional Meetings during the present week. But I may remark that some subjects, circumstanced exactly as I have described, will be brought under your notice by the reports which we have reason to hope for on the present occasion. Thus, the state of our knowledge of the laws of the motion of fluids is universally important, since the motion of boats of all kinds, hydraulic machinery, the tides, the flowing of rivers, all depend upon it. Mr Stevenson and Mr Rennie have undertaken to give us an account of different branches of this subject as connected with practice; and Mr Challis will report to us on the present state of the analytical theory. In like manner, the subject of the strength of materials, which the multiplied uses of iron, stone and wood, make so interesting, will be brought before you by Mr Barlow. These were two of the portions of Mechanics the earliest speculated upon, and in them the latest speculators have as yet advanced little beyond the views of the earliest.

I mention these as specimens only of the points to which we may more particularly direct our attention. I will only observe in addition, that if some studies, as, for instance, those of Natural History and Physiology, appear hitherto to have occupied less space in our proceedings than their importance and interest might justly demand, this has occurred because the reports on other subjects appeared more easy to obtain in the first instance; and the balance will, I trust, be restored at the present meeting. I need not add any thing further on this subject. Among an assembly of persons such as are now met in this place, there can be no doubt that the most important and profound questions of science in its existing state will be those which will most naturally occur in our assemblies and discussions. It merely remains for me to congratulate the Association upon the circumstances under which it is assembled; and to express my persuasion, that all of us, acting under the elevating and yet sobering thought of being engaged in the great cause of the advancement of true science, and cherishing the views and feelings which such a situation inspires, shall derive satisfaction and benefit from the occasions of the present week.

PROJECTED EXPEDITION INTO CENTRAL AFRICA*.

We learn from a "Prospectus" laid before the public, that Dr Smith proposes to direct in person a Scientific Expedition into Central Africa, with the view of "elucidating the Geography of these Regions, the nature of their productions, and the advantages they may offer to Commercial enterprise." The sum required to fit out the expedition is to be raised in shares of L. 3 each, and the "collections" will be the property of the Shareholders.

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* Sir James Macgrigor, Director-General of the Army Medical Department, had the goodness to communicate to us the *South African Journal*, from which this interesting account of the projected expedition into the Interior of Africa is extracted.—Edit.

It appears that two traders, named Hume and Muller, advanced to a point last year which they guessed to be near to or within the Tropic. Dr Smith's intention, we understand, is to penetrate if possible as far as the Equator.

Since the discovery of the Mouth of the Niger, a shorter route than any hitherto known has been opened to enterprising explorers, into the dark interior of the African Continent. But the route northward, from the frontiers of this Colony (Cape of Good Hope), is in many respects preferable. Here the traveller starts from a healthy climate, which accompanies him unchanged in this respect as far as discovery has yet reached. Should regions of swamp and miasma require to be traversed before the destined spot is attained, he knows that the restorative qualities of a purer atmosphere will increase with every day's journey on his return. He thus meets his greatest obstacle in full vigour, and should he or his party begin to suffer, Hope supports and soothes them with promises which can be quickly fulfilled.

The route from the western coast, where so many travellers have perished, is exactly the reverse of this. There they have been at once plunged into the bosom of fever and dysentery, by which they were weakened and reduced almost to despair before the business of discovery could be begun. In Mr Park's last Journey, on his arrival at the Niger, he found that, of *forty-four* persons whom he had brought with him from Pisania, there remained only *six* soldiers and *one* carpenter, all in the most infirm state of health, and one of them deranged. And shortly afterwards *five* more of the party died, amongst whom was his companion and friend Mr Anderson.

By proceeding from our frontier, near a line drawn from north to south through the centre of the Continent, the travellers, besides avoiding the regions of pestilence, will fall in with a less formidable class of native tribes than are to be found near the coasts, and especially near the great rivers where the slave-trade has for centuries converted the barbarian into a savage, and the savage into a demon. As far as the missionaries and traders have yet explored in this direction, the natives seem to regard the stranger neither with fear nor hatred. Repeated journeys of great length have recently been made among tribes hitherto unknown, even by name, in every direction northward of Lattakoo, which have terminated with scarcely any accident deserving the name of an adventure, forming a striking contrast with those attempted nearer the coast on the eastern side by Cowan and Denovan, Farewell, and others. Dr Cowie and Mr Green fell a sacrifice to the climate. Farewell's murder was partly the effect of revenge directed against himself and Chaca, with whom he had formed too close a connection to render his passage among hostile tribes prudent. Cowan and Denovan were lost by an act of great carelessness, having, in the presence of a dangerous tribe, divided their little party into three divisions, which were separately surprised and cut off in an instant. But the fact that a single mistake proves fatal, shows the dangers of the route.

The natives in the interior have never yet come into hostile contact with Europeans. According to the best accounts, they are comparatively tranquil, mild, and even courteous to strangers, though they carry on war against each other with great ferocity.

On the north the jealousy of barbarous nations inflamed by religious hatred, has almost sealed up this continent against discovery by Europeans. Solitary travellers have been cut off one after another, and it seems impossible to conduct any armed body of men sufficiently large to *act* in self-defence, across the deserts.

In every respect, then, we may consider the route from the frontier of the Cape Colony directly northward, as beset with the fewest *known* dangers. The *probable* difficulties and hazards, if we judge from what has been already discovered, are also much less formidable than those which travellers must prepare themselves to meet in other quarters.

The field of research is extensive. From the 32° of South Latitude to the Northern Tropic, our maps present us with almost a perfect blank. This comprises, perhaps, not less than one-third or one-fourth of the whole continent. And, as we have "*always something new from Africa*," a successful expedition, even as far as the Southern Tropic, can scarcely fail to increase the sum of the naturalist's stores, while it will afford useful information not only to the trader, but also to the civilized communities now forming on the extremities, and on the coasts of Africa. At present we are very much in the dark as to the risk we run of being visited sooner or later by some "*powerful Conqueror from the Interior.*" That such a personage may spring up is rendered more credible than we have hitherto considered it, by the recent acquaintance formed with Dingaan and Massalakitze. Our apprehensions are not strong of any very formidable attack from these chiefs, or from any resembling them in the interior. But it is well to know the character and resources of all who may hereafter become our allies or our enemies *.

FORMULÆ FOR TRIGONOMETRICAL SURVEYING. *By Mr* WILLIAM GALBRAITH, *A.M. Communicated by the Author.*

Some time ago, it was proposed to me to undertake a series of levels, at properly selected parallels, across the country, and to delineate the sections of these, to serve as a basis for a Geological Survey and Map of Scotland. In consequence of this proposition, my attention was directed to investigate and record formulæ and rules, for the purpose of fixing the positions of different points, with regard to latitude, longitude, and elevation, above the mean level of the sea. The instruments chiefly employed for this purpose, are generally the theodolite, the sextant or reflecting circle, the spirit-level, and mountain barometer. On many of the mountains and islands of Scotland, it occurred to me that the dip-sector, when both horizons are visible, might also be advantageously employed; and some exertions were made to provide as many of these different instrume[illegible] as might be required to perform the necessary operations in an accurate manner, so that the final results might be worthy of confidence.

Problem I.

To determine the elevation of a given point, by the depression of the horizon of the sea.

In my Mathematical Tables, Example xix. Plane Trigonometry, I have shown that the logarithm of the depression d, in seconds to the height in feet h, is expressed by the following formula:

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* We trust that ere this Dr Smith's expedition has started from the Cape. From the zeal of the conductor we expect much, more especially as, from the well known liberality of Sir James Macgrigor, he travels unfettered.

$$\log d = \tfrac{1}{2}\,(3.609990 + \log h) = 1.804995 + \tfrac{1}{2}\log h \,\ldots\ldots.(1.)$$

In natural numbers, this becomes $d = 63''.8256\sqrt{h} = 1'.064\sqrt{h}$.

If the general effect of refraction be taken at 0.08 of the arc, then $d = \frac{1.064}{1.08}\sqrt{h}$; and consequently

$$h = 1.031\, d^2 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2.)$$

in which d is the depression in minutes of arc.

This formula, for practical purposes, may be thus expressed:

$$h \doteq d^2 + \frac{d^2}{30} \text{ nearly}, \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3.)$$

or the height in English feet is equal to the square of the depression of the horizon of the sea in minutes, increased by one-thirtieth part nearly.

This is applicable to observations made with the dip-sector, the theodolite, and the spirit-level provided with a divided scale on a slip of mother-of-pearl, when within the limits of the instrument.

To those possessing fine instruments reading to seconds, the following rule, derived from the same principles, will be useful;

To the constant log. 6.456858, add twice the log. of the depression in seconds; the sum will be the log. of the elevation in English feet.

Problem II.

To determine the height of any point from a known horizontal base, and the angles of elevation at each extremity in the *same vertical plane*.

Let a be the base, and α and β the angles of elevation at the extremities; then,

$$h = \sin\alpha \sin\beta \operatorname{cosec}(\alpha \pm \beta)\, a \ldots\ldots\ldots\ldots\ldots (4.)$$

The upper sign must be used when α and β are both interior, the under when α is exterior and β interior.

In this latter case, it may be remarked, that the height h is equivalent to the base a, divided by the difference of the natural cotangents of α and β. This formula will also determine the perpendicular breadth of lakes, rivers, &c. when the angles are horizontal.

Problem III.

To determine the height of a point from a horizontal base, when its extremities and the point are not in the same vertical plane.

Let A and B be the horizontal angles measured at the extremities of the base a, and α and β the corresponding angles of elevation; then

$h = \sin B \tan\alpha \operatorname{cosec}(A+B)\, a = \sin A \tan\beta \operatorname{cosec}(A+B)\, a \ldots (5.)$

If the base a makes an angle θ with the horizontal line, then

$$h = \sin B \cos\theta \tan\alpha \operatorname{cosec}(A+B)\, a + \tan\theta a \ldots\ldots\ldots\ldots (6.)$$

above the lower extremity of the base, in this case, at A.

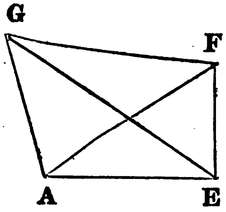
$$h = \sin A \cos\theta \tan\beta \operatorname{cosec}(A+B)\, a + \tan\theta a \ldots\ldots\ldots\ldots (7.)$$

above the lower extremity, supposed B in this case.

The last three formulæ will compute depths instead of heights, when the angles α and β become depressions.

Problem IV.

To determine the distance between two inaccessible points, when the angles between a given base and each of the points at both extremities of the base, are given.



Let AE be the given base a, and GF the required distance b, GAE $= \alpha$, FAE $= \beta$, AEF $= \gamma$, AEG $= \delta$.

Whence AGE $= \theta$, and AFE $= \varphi$: hence,

As sin AGE : sin GAE :: AE : GE = AE sin GAE cosec AGE,

As sin AFE : sin EAF :: AE : EF = AE sin EAF cosec AFE,

But Euclid II. and 12, $GF^2 = GE^2 + EF^2 - 2\, GE \,.\, EF \cos GEF$, which, by substitution, becomes

$$GF^2 = AE^2 \sin^2 GAE \operatorname{cosec}^2 AGE + AE^2 \sin^2 EAF \operatorname{cosec}^2 AFE - 2\, AE^2 \sin GAE \operatorname{cosec} AGE \sin EAF \operatorname{cosec} AFE \cos(AEF - AEG)$$

Hence,

$$b = a\{(\sin\alpha \operatorname{cosec}\theta)^2 + (\sin\beta \operatorname{cosec}\varphi)^2 - 2\sin\alpha \operatorname{cosec}\theta \sin\beta \operatorname{cosec}\varphi \cos(\gamma - \delta)\}^{\frac{1}{2}} \ldots\ldots\ldots\ldots (8.)$$

and conversely,

$$a = b \div \{(\sin\alpha \operatorname{cosec}\theta)^2 + (\sin\beta \operatorname{cosec}\varphi)^2 - 2\sin\alpha \operatorname{cosec}\theta \sin\beta \operatorname{cosec}\varphi\, (\gamma - \delta)\}^{\frac{1}{2}} \ldots\ldots\ldots\ldots (9.)$$

Similarly,

$$b = a\{(\sin\delta \operatorname{cosec}\theta)^2 + (\sin\gamma \operatorname{cosec}\varphi)^2 - 2\sin\delta \operatorname{cosec}\theta \sin\gamma \operatorname{cosec}\varphi \cos(\alpha - \beta)\}^{\frac{1}{2}} \ldots\ldots\ldots\ldots (10.)$$

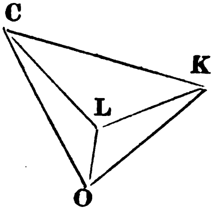
and conversely,

$$a = b \div \{(\sin\delta \operatorname{cosec}\theta)^2 + (\sin\gamma \operatorname{cosec}\varphi)^2 -$$
$$2\sin\delta \operatorname{cosec}\theta \sin\gamma \operatorname{cosec}\varphi \operatorname{cosec}(\alpha - \beta)\}^{\frac{1}{2}} \text{........} \quad (11.)$$

PROBLEM V.

To determine the same thing, if the given base have a parallel instead of a perpendicular direction to the required distance.

Let $OL = a$, $LOK = \alpha$, $LOC = \gamma$, $CKL = \theta$, $OKC = \psi$, $OLK = \kappa$: $CK = b$, $LKO = \beta$, $LCO = \delta$, $KCL = \varphi$, $OCK = \omega$, $OLC = \varrho$.



By a similar method of investigation,

$$b = a\{(\sin\alpha \operatorname{cosec}\beta)^2 + (\sin\gamma \operatorname{cosec}\delta)^2 -$$
$$2\sin\alpha \operatorname{cosec}\beta \sin\gamma \operatorname{cosec}\delta \cos(\theta + \varphi)\}^{\frac{1}{2}} \text{.........} \quad (12.)$$

$$a = b \div \{(\sin\alpha \operatorname{cosec}\beta)^2 + (\sin\gamma \operatorname{cosec}\delta)^2 -$$
$$2\sin\alpha \operatorname{cosec}\beta \sin\gamma \operatorname{cosec}\delta \cos(\theta + \varphi)\}^{\frac{1}{2}} \text{.........} \quad (13.)$$

Similarly,

$$b = a\{(\sin\kappa \operatorname{cosec}\beta)^2 + (\sin\varrho \operatorname{cosec}\delta)^2 -$$
$$2\sin\kappa \operatorname{cosec}\beta \sin\varrho \operatorname{cosec}\delta \cos(\psi + \omega)\}^{\frac{1}{2}} \text{......} \quad (14.)$$

$$a = b \div \{(\sin\kappa \operatorname{cosec}\beta)^2 + (\sin\varrho \operatorname{cosec}\delta) -$$
$$2\sin\kappa \operatorname{cosec}\beta \sin\varrho \operatorname{cosc}\delta \cos(\psi + \omega)\}^{\frac{1}{2}} \text{..........} \quad (15.)$$

There is another problem, of considerable utility in fixing the position of a point with respect to, and its distance from, three other given points. This is commonly known in this country under the name of Townley's problem, though it appears to have occurred to Snell a little earlier. It is given in most books on Trigonometry. I first extended it to the method of finding the latitude and longitude of a fourth point from the latitudes and longitudes of three other places. In this form, it has been applied by myself to fix the latitude and longitude of the Observatory of Edinburgh, when referred to three other points, whose latitudes and longitudes are given in the Trigonometrical Survey. These are given at full length in the second edition of my Tables, already referred to, and therefore it is unnecessary to insert them here. It has since been used successfully by a naval officer in a similar manner.

Example illustrative of Formulæ 8. *and* 9.

Let $\alpha = 139^\circ\ 15'\ 45''$ Whence $\alpha - \beta = 85^\circ\ 45'\ 22''$ $\theta = 8^\circ\ 55'\ 15''$
$\beta = 53\ 30\ 23$ $\gamma - \delta = 82\ 35\ 55$ $\varphi = 12\ 4\ 42$
$\gamma = 114\ 24\ 55$ $a = 6244{,}16$ feet.
$\delta = 31\ 49\ 0$ required b:

$\alpha = 139^\circ\ 15'\ 45''$ sin 9.814643 constant logarithm, or log 2. . . . 0.301030
$\theta = 8\ 55\ 15$ cosec 0.809473 $\gamma - \delta = 82^\circ\ 35'\ 55''$ cosine . . . 9.109982
0.624116 . . . . 0.624116
2 $\beta = 53^\circ\ 30'\ 23''$ sine 9.905215
No. 1, +17.71054 log 1.248232 $\varphi = 12\ 4\ 42$ cosec 0.679337
0.584552 . . 0.584552
2 No. —0.619680
No. 2, +14.76064 . . . . log + 1.169104 . —4.16562
No. 3, — 4.16562 . . . .
Sum, 28.30556 log . . 1.451872
half, . . 0.725936
$a = 6244.16$ feet, log . . 3.795474
$b = 33220.08$ feet, log . . 4.521410

Formula 10. would give the same results.

It is obvious, that if b had been given, it is only necessary to subtract half the log. of the sum from the log. b, to obtain the log. a. Now, in the course of our survey, in taking sections through the country, the distance of two places, derived from the Trigonometrical Survey being observed at any two consecutive stations, the distance of these would, by a computation similar to that above, be readily and accurately obtained.

Private individuals, however, in ordinary circumstances, cannot well undertake such laborious operations, which, unless accurately performed, can be of no real utility.

DR OPPENHEIM ON THE STATE OF MEDICINE IN EUROPEAN AND ASIATIC TURKEY. *(Concluded from page 273. of Vol. XV.)*

DR OPPENHEIM's other visits to harems were not always so agreeable as that just described, as appears from the following statement.

The Harem.—" The Pascha for several days in succession, required me to prescribe for a patient, whose name, sex, or age, he would not tell; and concerning whom I got no other information, than that the person complained of pain in the head and palpitations. I, of course, guessed that my patient was a female, but, nevertheless, for several days refused to prescribe, until told my patient's sex, age, appearance, &c. &c., alleging truly, that without being made acquainted with these particulars, my medicines might do more harm than good. My obstinacy extorted from the Pascha a confession, that she was a lady about thirty years of age, who had been ill nearly a year. Conjecturing that her ailment was hysterical, I prescribed nervous draughts; but finding she did not improve, I soon refused to send any more medicines. The credit I had obtained by the cure of Kiaja Bey's wife at last prevailed, and the Pascha acknowledged that my patient was his own wife, and consented that I should visit her. She resided at a considerable distance, and when I arrived at the gate of the harem, I was kept waiting outside for at least a quarter of an hour, in order that every thing might be arranged and properly concealed from observation before I entered. When the gate was opened, I was received by the matron of the harem, and conducted through the house to a door, when we again stopped; presently it was half opened, and two tall negresses wearing veils came out, and placed themselves one at each side of the door, which they again nearly closed, leaving space enough to admit one's arm. Through this aperture a hand made its appearance, emaciated, and painted of various colours; the nails were stained bright yellow, while each finger, as far as the first joint, was painted green on the back, and black on its other side. I felt the pulse for some time, but the moment I withdrew my finger from the wrist, the hand and negresses vanished, and the door closed. I was now called on for my diagnosis, prognosis, and plan of treatment. It was in vain that I requested the matron to make me acquainted with other particulars of the case; her invariable answer was, thou hast felt the lady's pulse, and must now know all! At last, after a persevering system of interrogatories and cross examination, I was enabled to elicit that the patient had a cough and expectoration, and above all that she was blind! It was for this blindness that she sought advice, and it was this disease that I was expected to recognise by feeling the pulse! I now explained to the Pascha, the utter impossibility of curing the defect of vision, without previously examining her eyes. He obstinately refused permission to do this, but still insisted upon my giving some medicines. With this request I complied at last, and sent powders composed of white sugar, &c. I did not fare much better in a visit I soon after made to a third harem. I was received with the customary ceremony, and treated with the usual hospitality; walking in the garden with the husband of the sick lady, I asked him concerning her symptoms. He answered that he knew nothing about them, but that the information I required was not necessary, for he intended to let me feel her pulse. I found her lying on the floor supported by pillows, like the lady I had first visited, but much more scrupulously covered and concealed from view, so that indeed it seemed scarcely credible, that such a mass of pillows, bolsters, and shawls, could be piled on a human being. I was not permitted to ask any questions, but when I was seated beside the patient, her husband said, The physician is there; whereupon the hand and arm, most carefully shrouded and covered, except at the wrist, where a bare space was left for the finger of the physician, were protruded from the mass of pillows. When I had felt the pulse I was conducted out, and it then took me more than hour to obtain answers to the necessary questions; for a special message had to be sent into the harem to inquire about each particular symptom and circumstance, such as the patient's sleep, the state of her bowels, thirst, uneasy sensations, &c. &c. In the end it turned to be a case of simple catarrhal fever, and yielded to a sal-ammoniac mixture *. During the three years and a half I practised in Turkey, I had at least an hundred attendances in harems, and have always found that the young and beautiful are the least careful of concealing themselves from view, while it was always impossible to get a glimpse at the more elderly and faded. When the veil is allowed to perform its duty undisturbed during the whole of his visit, the physician may be sure that the wearer's beauty is on the wane. The jealousy of the husband seems to increase with the number of his wives, and when his harem contains but one, the physi-

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* The muriate of ammonia is much used in Germany, in various febrile and chronic complaints. It appears to be a remedy deserving of attention, and I shall therefore take an opportunity hereafter of explaining its medicinal virtues.

cian is admitted with more facility. Still it is not in the power of any husband to prevent the visits of the physician, for let his jealousy be ever so vigilant, it will be eluded, and as a last resource, the lady has nothing to do but fall sick during her visit to the bath, and send for a physician; at certain periods the use of the bath is inculcated by the Mahommedan ritual, and no husband dare, therefore, debar his wives from this enjoyment. When a physician has a patient in a harem, it is his duty to send every morning to inquire how her ladyship (channem) has slept, &c. In answering this inquiry, the lady informs him at what hour it is her wish to see him. This ceremony goes on until the patient recovers, when the physician receives, in token that his services are no longer required, a present of a silk shirt, a pair of silk trowsers, an embroidered girdle and handkerchief, and a pair of knit socks. This present is a mark of the lady's gratitude. The Turks have a very different taste with regard to female beauty, from the inhabitants of the more northern parts of Europe. An excessive *en bon point* is an essential ingredient in the composition of a Turkish beauty, and in general, by means of taking little exercise, frequent bathing, and over eating, they attain at a very early age a rotundity of figure, which to a German eye appears any thing but attractive; black hair and black eyes, with black eyebrows running together over the nose, are highly valued, and when nature has not conferred these attractions, the aid of art is invoked, the eyebrows are stained, and the skin between them painted of an appropriate colour. A Turkish beauty must not have a single hair on any part of the body, except the scalp and brow. In the baths, their female attendants spend hours in destroying the forbidden hairs, not by eradication or shaving, but by means of a composition which softens, corrodes, and destroys them, after which the whole surface of the body is rubbed and polished by means of a powder, consisting of alum and other ingredients. When this process has been successfully and skilfully performed, and when the nails of the fingers and toes have been stained orange by means of the leaves of henna (Lawsonia inermis,) then the Turkish beauty is irresistible in the eyes of all true Moslems."

Turkish Cosmetics.—The Turkish ladies make use of a cosmetic which Dr Oppenheim strongly recommends to such of our fashionables as are in the habit of employing the deleterious substances called *rouge*. It consists of a peculiar preparation in the form of a powder, derived from the bulbs of the *Iris florentina.* The bulbs are dried and powdered, and the powder is macerated in water, after which it is squeezed through fine soft linen. This is twice repeated, and the residual powder which remains in the bag is carefully dried and kept in bottles. A little of this powder is placed on the cheek, and the part is then gently rubbed for several minutes with the palm of the hand; a sensation of heat in the cheek is thus produced, which is accompanied by a natural looking red blush, which lasts for several days without fading, and is not spoiled by perspiration or heat. The antimonial cosmetic which they call *surmeh,* is used for dyeing the eyelashes and edges of the eyelids, and they dye the hair of a beautiful black, by means of a paste formed with burnt nut-galls, boiled with vinegar, and applied very hot. The *pulvis depilatorius rusma,* so generally used in Turkey, consists of orpiment and quicklime, and is applied, as was before observed, for the purpose of destroying the hairs of the axillæ and other parts in females. It is formed into a paste with warm water, and laid on in a thin layer, which is then washed off in a few minutes. It does not destroy the roots of the hair, and consequently does not prevent its subsequent growth. Another preparation, used for the same purposes, and whose active ingredients are the same, is called *Oth;* in consistence it resembles very fine aluminous clay, and is made into a soft mass by means of trituration, with scented soaps and rose leaves.

Hanging a Company of Chemists.—Although moderation in both eating and drinking is very general among the Turks, and the use of wine forbidden by their religion, yet it does not appear that they are longer lived than other European nations. The inhabitants of Chios* indeed, both Turks and Christians, are celebrated for their longevity. Dallaway conversed with a bridegoom in Chios, who was 120 years of age, and whose son, then an octogenarian, had just been felicitated on the birth of a child. The belief is almost universal in Turkey, that such a thing exists as an *elixir vitæ*, and even Ali Pascha, the celebrated tyrant of Janina, employed a company of alchymists for five years in his fortress, during which time they worked diligently in search of an elixir capable of prolonging life. They failed, and Ali, enraged at the expense he had so foolishly incurred, hanged them all.

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* This beautiful island counted 90,000 inhabitants in 1822, before the devastations of the Turks, who destroyed 89,000.

"*Early Maturity of Women.*—The oriental women are like hot-house plants, which blossom quickly and quickly go out of blow; at ten they menstruate, at twelve they marry, and at thirty the catamenia cease. There is with them no intermediate stage, no twilight, to soften the abruptness of the passage from youth to old age."

Dr Oppenheim makes an interesting remark on this subject. He says that this early development depends not on climate so much as on the race of mankind. Thus among the Bulgarians, a Sclavonic race, who have settled in Turkey, the females menstruate late, and seldom marry before five-and-twenty or thirty, while, on the other hand, among the Jews, settled in the north of Europe, in Russia and in Poland, the females arrive at puberty two or three years sooner than the Sclavonic inhabitants of the same countries.

Turkish Population decreasing.—Although the Turks are naturally a strong healthy people, yet at the present moment, the population is far from being on the increase, a circumstance which may be accounted for by the prevalence of certain habits and customs, such as polygamy, the abuse of coffee, opium, and tobacco, &c. When to this we add the native blood shed so abundantly under a government long tottering and unstable, and the devastations of the plague, it is easy to credit the assertion of Dr Oppenheim, that the population of Turkey undergoes a considerable annual diminution. In Turkey the mortality among children is very great, in consequence of inoculation being much more generally practised than vaccination. In the Asiatic provinces, indeed, inoculation is almost exclusively employed, and this operation is intrusted to old women, who perform it very adroitly. A great number are carried off by the smallpox, which Dr Oppenheim partly attributes to the heating regimen of medicines employed for its treatment. He mentions, that in Tunis, it is usual, in this disease, to rub the whole surface of the body with fresh butter, in order to soften the skin and facilitate the appearance of the eruption, and he observes, that the custom so prevalent in hot countries of anointing the body with oil, fat, or butter, is founded upon the experience of its utility. Thus Buckhardt, the celebrated traveller, assures us, that when exposed to the heat of the sun, he experienced great refreshment from using oily inunctions, and when tired by a long day's journey, he used to feel benefit from rubbing his feet with butter. Dr Oppenheim reserves for his forthcoming work, numerous proofs of the utility of oily inunctions in the plague and in dysentery, and he remarks, that in military hospitals they have the additional advantage of destroying *pediculi*.

Scarlet Fever and other Diseases.—The scarlet fever commits frequent ravages in Turkey; the following remarks by Dr Oppenheim are well worthy of attention, and prove that the prophylactic virtues attributed to belladonna, merit further investigation *:

" At Monastir, in 1829, scarlatina raged, both among our troops and the inhabitants of the towns and villages where we were quartered. The grand vizier, who had expended much time and money on the discipline of this his favourite *corps d'armee*, gladly accepted my proposal to try the effects of belladonna. As the troops were generally very young men, and totally unaccustomed to narcotics, the dose I gave was comparatively small: thirty-six grains of the extract of belladonna were mixed up with one pound of the extract of liquorice, and ten grains of this were given morning and evening to each soldier. The success of the experiment far exceeded my most sanguine expectations, for not more than twelve men, out of twelve hundred, sickened after this plan was adopted; of these twelve six died, and it is to be remarked, that the disease continued unabated among the inhabitants where the soldiers were quartered, after it had ceased among the latter, although they lived in the same houses."

This experiment appears almost conclusive, and should encourage a further trial of the same practice. I had lately an opportunity of putting this prophylactic property of belladonna to the test, in the case of a young gentleman whom I attended along with Dr Jacob, and who was at the very crisis of a violent purulent ophthalmia with ulcers of the cornea, when scarlet fever made its appearance in the family. . In his extremely reduced state, a new disease would have destroyed his eyes, if not his life. We immediately put him and his brother, who was recovering from common fever, on the use of belladonna, and they both escaped scarlatina, while two other children were attacked. It is true, that after the first case of scarlatina had appeared in the house, our patients were separated from the rest as carefully as is possible to effect in a numerous family; the evidence is, therefore, so far incomplete, and indeed no positive inference can be drawn from an experiment on so small a scale.

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* *Brande*, in his Pharmacy, scouts the idea of its prophylactic powers.

The following interesting observation concerning the scarlet fever epidemic at Monastir, deserves notice:—

The disease invariably commenced with symptoms bearing all the characters of the true inflammatory type, and consequently some of Dr Oppenheim's colleagues were tempted to make use of very energetic antiphlogistic measures—measures sure to be followed by a typhous state, which generally ended fatally. A few years ago, it would have been very injurious to any writer's reputation even to hint at a typhous state being induced or accelerated by antiphlogistic treatment in the commencement of any disease whatsoever. The Brunonians, who treated all febrile diseases with stimulants and opiates, were scarcely more destructive practitioners than their lancet-wielding successors. As in most controversies, truth lies between the two extremes. *It is singular enough that Dr Oppenheim did not see a single case of measles during his residence in Turkey;* nor had any of his medical acquaintances, several of whom had practised many years in that country, ever witnessed one.

Urticaria and herpes, particularly in the form *herpes zoster*, are very frequent in Turkey, as are also miliary and petechial eruptions, neither of which are dangerous, unless when symptomatic of the plague.

Of chronic cutaneous complaints, the most common are impetigo, porrigo, and scabies, all of which are treated by the popular remedies, oily inunctions and sweating baths. Worms, and the diseases they produce, are very common among the Turkish children. A popular remedy is a powder formed of the kernels of wild apricots, a fruit which grows in great abundance in Asia Minor. A decoction of the seeds of the *Ricinus*, which Dr Oppenheim says is indigenous in Turkey, is also used in worm complaints. Tape-worm is very common, and every town has its quacks, who vend specifics for its expulsion; and, in truth, as Dr Oppenheim testifies, they often succeed. It is from the Turks that we have lately learned the properties of the *Brajera anthelmintica*. Acute hydrocephalus is almost universally mistaken for worms, and is by no means unfrequent. At Janina Dr Oppenheim performed the operation of paracentesis in the case of a boy a year and a half old, who laboured under chronic hydrocephalus. The operation excited the astonishment of the whole harem, and was repeated six times at intervals. The head diminished in size remarkably, and the child recovered—a very rare but not unexampled occurrence. Hooping-cough is very common and very fatal, particularly when it coincides with dentition; scarification of the gums is very frequently resorted to when dentition is difficult and painful. Scrofula, rachitis, bowel complaints, tabes mesenterica, and disproportionally protuberant bellies, are much less common among Turkish children than in other countries of Europe, a circumstance which Dr Oppenheim attributes to their using a more nourishing diet, consisting chiefly of rice, and to the freer ventilation which the mildness of their climate permits them to enjoy in their houses. Sedentary habits, an immoderate use of strong coffee, and an over indulgence in sensual gratifications, render the males, among the higher classes of Turks, very liable to hypochondriasis, while among the females of the harem, hysteria reigns with despotic sway.

" The chief causes of hysteria are a sedentary life and confinement, restraints with respect to sexual intercourse, the too frequent use of warm baths, the interference of unskilful midwives during parturition, and the too common habit of taking medicine to procure abortion. Barrenness is the greatest misfortune that can befall a Turkish wife, as it entails contempt on the part of acquaintances and neglect on the part of husbands, which often ends in their putting away their wives, an irreparable calamity, for a woman divorced for such a cause, is looked upon as unsound and degenerate, and can never expect to get another husband. Hence, the harems are head quarters for all manner of contrivances and means supposed capable of promoting female fertility; and the physicians, old women, and midwives drive a lucrative trade in this branch of science. Injections, washes, and suppositories are successively tried, and in this manner introduce the most stimulating substances, such as aloes, myrrh, musk, bezoar, cinnamon, opium, &c. &c. As might be expected, these means often defeat the hopes of the misguided female, and produce abortion.

"Abortion is very often brought on intentionally, and such attempts are legal and sanctioned by their ritual, provided the fœtus is under five months old, for at that period the fœtus is supposed first to have life! Hence, married women frequently ask their acquaintances or physicians for medicines to procure abortion, and do not appear to think they are doing wrong. Their object in such cases is to gratify their husbands when unwilling to have more children, or when they fear that another parturition and nursing would spoil the personal appearance of their ladies."

Uses of Bezoar Stones, &c.—The Turks still place unlimited confidence in the efficacy of Bezoar stones, an article so much used that to replace the natural concretions, found in the stomachs of certain animals, Pseudo-bezoars exist in Constantinople. This remnant of oriental superstition has not been long expelled from the British Materia Medica, and such stones brought a high price in London less than a century ago. The tears of Turkish saints are in good repute, but, being scarce, are administered only in extreme cases, and in very minute doses! I know not whether it may increase the reputation of animal magnetism among the Parisian philosophers, to be told that it is a favourite remedy with the Arabian dervises and jugglers.

The orientals place much reliance on the efficacy of saliva in curing diseases, and Dr Oppenheim was forced frequently not merely to give proper medicines, but to spit on the affected part, a superstition derived from the circumstance of most of Mahommed's miraculous cures having been wrought by the direct application of saliva.

Ague.—Ague, in all its forms, is common in Turkey; in the Asiatic provinces it is universally believed to be the result of demoniacal possession, and exorcism is considered the surest remedy; but in the European provinces and chief towns, sulphate of quinine is preferred.

A popular medicine, which Dr Oppenheim has often seen effectual, is the following:—Half an ounce of fresh roasted coffee is infused with two ounces of hot water, and is afterwards mixed with an equal quantity of lemon juice. This dose is taken warm during the intermission. *Her Gueinlueck zitmai*, *zitmai mueselllesse*, and *zitmayi rib*, are the names given to quotidian, tertian, and quartan. *Gout (wedjai mefassel,)* and *rheumatism (nikris)* are also attributed to demoniacal possession, but, luckily, in these cases, the physical means employed to expel the evil spirit, such as pinching, kneading, and pummelling the patient's flesh, stretching his limbs, cracking his joints, together with the warm bath, &c., often act extremely well, and promote a real cure.

Gout not common in Turkey.—Gout is not near so common in Turkey as in other European states, and the physicians employ for its cure no remedy but the warm bath. Hippocrates, in his Aphorisms, asserts, that eunuchs are never attacked with gout; but Dr Oppenheim says, that he never witnessed so painful and violent an attack of podagra, as he saw in an eunuch, a water drinker, and eschewer of opium to boot; in fact, a perfect model of sobriety, temperance and chastity!

Erysipelas in Turkey.—It is curious enough, that the treatment of erysipelas by stimulants, such as the nitrate of silver, blisters, &c., lately introduced into British practice, is employed also by the Turkish common people, who, however, use a stimulant of a different nature, viz. heat. Some spread a silk handkerchief tightly over the affected part, and cover it with very porous shreds of cotton, which they set fire to; they are consumed, but the handkerchief escapes *, and during the combustion of the wool, a sensation of heat and pain is felt in the erysipelatous portion of the skin: others prefer puncturing the erysipelas in many points, with sharp spiculæ of wood, and immediately applying to each puncture the point of a burning stick. Dr Oppenheim asserts, that he has seen both these methods successfully employed, for the purpose of *fixing* the erysipelas, by which, of course, he means that the disease was prevented from spreading.

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* On the same principle that a red coal, placed on a handkerchief, stretched over a poker, does not burn the handkerchief.

Dogs do not become mad in Turkey.—Dr Oppenheim corroborates the testimony of those who assert, that dogs never became mad in Constantinople or Egypt, although the prejudices of the people allow the canine species to multiply in a manner quite anti-Malthusian. He remarks, that the dogs and birds of prey, the scavengers of the East, are justly entitled to the regard of the philosophers, for without their aid the "land of the sun" would soon become uninhabitable, so averse are the Turks to the removal of nuisances.

Standard of Morality among the Turks.—Dr Oppenheim quaintly observes, that every city in Greece claimed to be the birthplace of Homer, but every country in the civilized world refers the origin of syphilis to its neighbour. So is it with the Turks, they say it is a disease altogether of Frankish origin. It is extremely diffused, particularly in the towns, where it affords a rich harvest to the quacks, who profess to cure the disease by warm baths and purgatives, continued for twenty or thirty days in succession. The regular physicians do a vast deal of injury, by an injudicious use of mercury, which they exhibit both internally and externally, in the form of calomel, corrosive sublimate, red precipitate, &c., in frightfully large doses. The chancres are in general touched with lapis infernalis, or with arsenic. The frequent employment of the bath and circumcision, to a certain degree, prevent the effects of impure coition, a source to which the Turks are extremely unwilling in any case to attribute the disease. Thus, a gonorrhœa is always accounted for by their catching cold, as indeed the name of the disease *belzouk*, implies (a cold in the back, from *bel*, back, and *zouk*, cold). Their treatment of gonorrhœa is worthy of notice, but not of imitation, for they give cantharides in the form of pills or tincture, in such considerable doses, as often to produce violent irritation of the urinary organs: they are likewise in the habit of recommending astringent injections in this disease. Notwithstanding this injudicious practice, strictures of the urethra are very rare, a circumstance worthy of observation. Dr Oppenheim, in his remarks upon the venereal disease, discloses many circumstances, which prove the almost incredible prevalence of unnatural vice in Turkey, not only among the ignorant and uneducated, but among the highest functionaries, of the state. The late, and the present Grand Vizier, Redschid Mehmet Pascha, are quoted as examples; in short, it is evident that the standard of morality among the Turks is not higher than it was among the Romans in the time of the emperors, whose tyranny was well deserved by subjects totally destitute of private virtue. If other proofs were wanting of Turkish sensuality, the enormous prices they give for aphrodisiacs would alone establish the fact.

Eaters of Opium, and of Corrosive Sublimate.—Dr Oppenheim saw a large price given in Cæsarea, for a powder, made by incinerating the brains of male and female sparrows in equal quantities. The Doctor makes some very interesting observations on the practice so prevalent in Turkey, of eating opium, to which is added, in confirmed cases of this dreadful habit, corrosive sublimate. Some take the latter poison to the extent of ten grains daily. *Strohmayer* is quoted, as having lately published in Vienna, an account of a Tyrolese peasant, who was in the habit of mixing ten grains of arsenic with his daily portion of food. Quite as extraordinary is the fact I have lately seen in some work, that in one of the islands of the Pacific, the inhabitants have no other water to drink but sea water. Mackenzie or Hooker, I forget which, relates that in Iceland, the cows are often fed with dried fish during the winter! Those who indulge in opium, always avoid drinking water or any fluid at such times, as so doing often occasions colic. It is particularly worthy of notice, that Dr Oppenheim himself was attacked in Turkey with intermittent fever, which at first had the quotidian form, but soon changed its type, so that the paroxysm came on every seventh, and afterwards every fourteenth day. In one of the clinical lectures, published in the London Medical and Surgical Journal, I have noticed this strange species of ague, as occurring in some parts of Russia.

The following account of the effects of opium is extremely interesting:—

"In the month of May 1830, I attended Abduraman Pascha, on his journey from Koniah to Kütaja. We bivouacked one night near a little village called Karako, and, as usual, I slept without covering my head as the Turks do, when they spend the night in the open air. Next morning my dragoman awoke me, just in time to depart with the rest; but I was unable, being affected with a violent pain in the head, and feeling myself stupid and giddy like a drunken man. I heard the voices of those around me, but did not understand a syllable of what they said; I could not speak, and again fell asleep. In a few minutes I was astonished by a violent shock and dashing of cold water on my head; this aroused me, and I found that my companions had hastened to bring up one of the camels, and had emptied one of the skins upon my head, thus promptly and ingeniously finding a substitute for a shower bath. In fact I had been in a state of narcotism, induced by the neighbourhood of an opium field, in which the poppies were in full blow. In the course of an hour, I was enabled to pursue my journey. The occurrence shewed me, that they were long aware of the utility of the cold dash in Turkey, a practice only lately recommended in Germany, on the authority of the English physicians Copland and Wray, in cases of poisoning by narcotics. On another occasion I slept near Brussa in an open field to leeward of a large caravan of camels, laden with opium, and again I awoke in the morning with nearly the same symptoms."

Insanity and Suicide in Turkey.—Dr Oppenheim's observations upon the causes which render insanity and mental derangement extremely rare in Turkey, are very interesting, and well worthy of being studied by those who devote themselves to medical statistics. Here it is sufficient to remark, that their habitual suppression of the feelings of extravagant joy or sorrow, the fatalism inculcated by their creed, the contempt with which they regard all scientific or practical improvements, their total ignorance of passion-stirring literature, the absence of all scepticism with regard to the truth of the Koran, and the want of the sentimental feelings of love; all these circumstances combined, cause each individual to look upon the wants of life with comparative apathy; in consequence of which, suicide and madness are almost unknown in Turkey. Suicide is most frequent in highly civilized nations, where murder is rare; while in countries where ignorance prevails, murders are very frequent and suicides rare. France is an example of the former, Ireland of the latter. It is excessively rare to hear of a suicide in Ireland: an Irishman indeed scarcely ever thinks of killing himself, but he frequently kills his neighbour.

The only examples of persons committing suicide Dr Oppenheim met with in Turkey, were three, two of whom were Christians, and one a renegade. Of the degraded state of Christianity in the East, no one can form a notion who has not witnessed the ignorance and superstition of the priests of the Greek church: Tournefort's Travels in the Levant, elucidate this assertion most amply. Among these Christians too, insanity is much more frequent than among the Turks; a fact which clearly proves, that the fatalism inculcated by the Koran exercises a decided influence over the mental constitution of the Moslems. Religious madness is unknown among them, for their religion consists, not of spiritual doctrine, but of blind obedience to a code of laws, which embraces, not motives, but mere actions, and which promises the enjoyments of Paradise to all those who believe in God and his prophet Mahommed. A non-observance of these laws, and occasional criminality, may retard, but cannot finally prevent their entrance into Paradise. They pass their lives in the fancied security of ultimate happiness, and, consequently, they consider the misfortunes of this life, as transient and unworthy of exciting deep or lasting exertions. Still the feelings of nature cannot be wholly exiled from the human breast, and the Turk, who regards the near approach of personal misfortunes, or even death, with calmness and apathy, often exhibits symptoms of most intense feeling, when a beloved child or wife is attacked with an acute and dangerous disease. Then he calls in the assistance of every physician he can procure, and to remunerate them he is willing to make the greatest sacrifices: should the disease continue, however, for many days, his religious tenets resume their wonted sway, the physicians are dismissed, and the patient is resigned into the hands of destiny. Some new medical man comes into the neighbourhood, when again the voice of affection is listened to, and the immutability of fate for the time forgotten. Sceptics in religion are scarcely ever met with in Turkey; Dr Oppenheim witnessed but one instance; he was in attendance, during the last few days of his illness, upon Œmar Effendi of Kütaja; he was not afraid of death, but he was very anxious to know exactly how long he had to live. "Shall I," said he, "see the sun rise and set tomorrow?" "You cannot live until sunrise," replied Dr Oppenheim. "Then before sunrise I shall know whether Mahommed is an impostor!"

Idiocy produced artificially in children and adults.—Although insane persons are so rare, naturals and idiots are not so unfrequent; and occasionally idiocy is produced in children artificially, by means of giving the child small doses of narcotics from its very infancy; a practice which, by stupefying the sensorium, prevents the mental development, and ends by producing a state of fatuity. This is an extremely curious fact, and I believe that Dr Oppenheim is the first who has given us authentic information upon the subject. This practice of rendering persons idiotic, is the source of great emolument to some; and Dr Oppenheim says, that it is carried into effect, not merely upon children, but upon adults, when it is judged necessary to render them incapable of conducting their affairs, while, at the same time, their removal by death appears, for certain reasons, impolitic and inexpedient.

Practice of the present Sultan.—The present Sultan is said to have had recourse to this infamous proceeding, in the case of his son and heir apparent to the throne, Abdul-Medsched, a boy nearly thirteen years old; he committed this act, lest the Janissaries and their friends might seize an opportunity of dethroning himself, and of elevating his son in his stead, a fear which had led him, at a former period, to sacrifice his eldest son, then a boy of tender age. The reigning Sultan is the last of his father's thirty children, and is the sole remaining descendant of the family which sprung from Mahammed, that has hitherto been in hereditary possession of the throne. The loss of such gentle blood would be irretrievable, and therefore it is to be hoped, that some of the kings and emperors, who on all sides press forward to prop up the falling fortunes of the Sultan, will be able to persuade him to abandon this Saturn like-propensity.

Lunatic Asylums the only Hospitals in Turkey.—The only hospitals of any sort in Turkey, are those appropriated to the reception of idiots, institutions better regulated than could be expected, in a country otherwise so backward in civilization. The inmates are persons afflicted with fatuity or epilepsy, and the idea, so universally prevalent, that these diseases partake of a sacred character, operates very favourably in increasing the amount of alms contributed to their support, and consequently these hospitals abound in Turkey.

State of Surgery in Turkey.—Surgery has hitherto scarcely deserved that name in Turkey, and indeed it could not be otherwise, in a country where dissection, or even opening a dead body, is expressly forbidden by the Koran: "thou shalt not open the body even of a criminal, who has stolen and swallowed pearls of price," are the emphatic words of the prophet. The present Sultan has caused to be published in Constantinople, a large folio, a sort of cyclopædia of practical medicine, surgery, and anatomy, with anatomical plates, which is used in the medical school established under his auspices, but is ill understood by the pupils, on account of containing many Greek and Latin phrases, either untranslated or translated very badly.

A Turkish surgeon has no instrument but a lancet, scissors, searing iron, and a forceps for extracting balls. Except bleeding and cupping, (in the latter they are great adepts), they never perform an operation, as they dread hemorrhage, which they are totally unable to control, their only resource being the application of some styptic powder, or else the iron ore, called hematite, to the bleeding part. In some cases they sear the wound with the actual cautery: when the limbs are chopped off by the executioner, the only person who ever performs an amputation in Turkey, the criminal's friends arrest the bleeding in this manner. He who is caught in the act of thieving, has his hands cut off; the Koran is explicit in enjoining this punishment. For slight offences, they nail the ear to a post, at such a height that the sufferer just touches the ground with his feet, a position by no means enviable. Scarifications of the skin are frequently used and often with great benefit: they apply them to relieve pains in all parts of the body, and use them in persons of all ages. I have known this operation to be performed in the country parts of Ireland, with great benefit, in a bad case of sciatica: the operator made twenty or thirty very superficial but long incisions in the skin, close to each other, so as nearly to cover the calf of the leg; much blood flowed from the wounds.

Abscesses the Turks treat with poultices, made of figs and honey, and never venture to open them. They pour oil or melted butter into gunshot wounds; inflamed and painful wounds, they cover with the hot flesh of an animal recently killed, or else apply to them an ointment, made on the spot, by beating up white lead with eggs. When an ulcer is relaxed or indolent, they lay chewed figs upon its surface, which they sometimes sprinkle with a little arsenic or red precipitate. In cancerous sores, they are acquainted with the use of animal charcoal, which they applied, not in substance sprinkled over the sore, but made into a liniment with oil. Being totally unacquainted with the use of either charpe or lint, they apply cotton, in the shape of dressing, to sores; a practice which, as Dr Oppenheim remarks, has deservedly found advocates in Germany, as well as England, particularly when the sores are extensive, and the process of scabbing may be attempted with a prospect of success. Dr Oppenheim says, that he has found nothing so effectual or so convenient for the cure of the ill-conditioned surfaces, occasionally produced by blisters, as dressing the part with very fine cotton-wadding; this must be kept applied to the part by means of a bandage; the exudation from the blistered surface soon concretes, and causes the cotton to stick closely, forming a sort of scab, under which the part often heals without further trouble. This process has been likewise adopted with great advantage, in cases of extensive scalds and burns, in our manufactories and hospitals, and is founded on most rational principles. In cases where the external surface is extensively denuded by wounds, I have little doubt that the process of scabbing might be often facilitated by a similar treatment. Poisoned wounds the Turks suck, excise, and cauterize. There are many of them who profess only a single branch of surgery, as bone-setting, operators for cataract, hernia operators, lithotomists, &c. &c.; such persons often enjoy a great reputation, which is handed down from father to son. They observe strict secrecy as to their modes of operating, and boast unceasingly of their success. Dr Oppenheim does not deny that, on many occasions, he witnessed results which proved them to possess considerable expertness; this is true, for example, with regard to the professed bone-setters of our own country. Here, as in the East, they often do irretrievable mischieve. The quack is never blamed, however, in such cases; this is their privilege all over the world. The Turkish bone-setters use bandages and splints innumerable, and have the merit of having introduced the very curious method of keeping the ends of broken bones in apposition, by means of plaster-of-paris, applied soft, and left on until it sets, and thus secures the proper position of the limb. The Turkish surgeons hold amputation in abhorrence; and so great was the prejudice of all ranks, from the soldier to the vizier, against this practice, that Dr Oppenheim was unable to introduce it even in cases of gunshot wounds, with comminuted fracture, &c. &c. Cases of this nature, altogether desperate and hopeless, unless amputation was immediately resorted to, the Turkish surgeons promised to cure without it; and because Dr Oppenheim could not promise with certainty that the patient would, in every case, recover, he was soon prevented from performing the operation in any ! !

As to operations after battles, in the Turkish campaigns, none were performed on the wounded prisoners, as they were universally beheaded the next day, by command of the vizier, to whose army Dr Oppenheim was attached; occasionally, the clemency of the conqueror was exhibited in rather a singular way, for he ordered the prisoners who were wounded in the back to be spared, as an encouragement to flying enemies ! ! On one occasion, after the battle of Monastir, the wounded and other prisoners were included in a treaty, and twenty Turkish piastres a head were paid by the Albanians as ransom for their captive countrymen. As soon as the money was in the pocket of the vizier, he beheaded the wounded, and sent the rest to be sold as slaves at Constantinople * ! Tooth-drawing is entirely in the

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* An able writer in the Dublin University Magazine for July last, has taken upon him the defence of the moral character of the Turks and of the Sultan, and invokes Europe to protect the latter from the barbarous Egyptians ! He speaks of the Sultan's paternal affection for his children, and asserts that his eldest son died of the small-pox. Let him read Dr Oppenheim's

hands of the barbers, who, in Asia Minor, may be seen in the streets employed in boiling coffee, which they sell by the cup, or in drawing teeth with the forceps, the only instrument they use. Hernia is very frequent in Turkey, and often proves fatal: the early age at which they all practise horsemanship, their bad saddles and bad roads, make them liable to be violently shaken in riding; this probably accounts for the frequency of hernia. There are no trusses made in Turkey, and consequently the poor are totally unprovided with them; a few of the wealthy procure them from Vienna, France, or Italy, but these, in the course of time, being repaired in a bungling manner, look like any thing but trusses, and injure rather than serve the wearers. The Turkish surgeons are totally unacquainted with the operation for freeing strangulated hernia, but they frequently attempt the radical cure of ruptures, by means of ligatures or the actual cautery.

"I had an opportunity of witnessing at Jenetschär (Larissa), this operation performed by surgeon Michalaka of Sagor. The patient was a robust man, about forty years old, who had the hernia for many years, and was now resolved to get rid of it, on account of the inconvenience it caused when he rode. When the operator had convinced himself that the gut could be easily returned, he tied the patient on a board, forming an inclined plane, so that the patient's feet were much higher than his head.

"With one hand he pressed against the neck of the sac, so as to prevent the gut from re-entering it, with the other he made an incision into the tumour, extending from about one inch above Poupart's ligament, to two inches below it. He thus brought to view the proper hernial sac, or, as he termed it, the bladder of the rupture. This he pulled forcibly, with both hands, out as far as possible, tied a strong silken string round the neck of the sac near the ring, and cut away the sac below the ligature. The spermatic chord was evidently included in the ligature.

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book in the original, and I think he will feel as little inclined to plead the cause of Sodom and Gomorrah, as that of the Sultan and his Paschas; I am glad to see the true character of the Sultan exposed, in a letter from Constantinople, published in the Standard of 8th July.

which I remarked to him; but he stoutly denied the possibility of his having committed so unfortunate a mistake."

Generally, this gentleman, and others who perform similar operations, avoid including the spermatic chord in the ligature, by pushing both it and the testicle into the abdominal cavity. The ancient cruel method of using the actual cautery, to produce the inflammation necessary to close the hernial aperture, is now seldom resorted to. Some of the rupture doctors affect all the state of our ancient quacks; when they enter a town, they proclaim their approach with sound of trumpet, and ostentatiously display, like a standard, a long pole, from which hang the numerous hernial sacs they have amputated.

Hernia and the stone, probably on account of the pain they occasion, are the only two diseases in which the Turks permit operations. *Calculus* is very frequent in some provinces of Turkey, particularly Macedonia, Epirus, and Thessaly; and Dr Oppenheim mentions several instances where the disposition to the disease seemed to be hereditary. The operation of lithotomy always used is that described by Celsus, formerly called *cutting on the gripe*, or the *lesser apparatus*. The after treatment is altogether neglected, antiphlogistic remedies never applied, and the absurd regulation enforced of keeping the patient from sleeping for four-and-twenty hours after the operation. This is effected by means of music and various noises, perpetually kept up in his chamber. Unfavourable as are all these circumstances, the mortality is by no means so great as might be expected, and Dr Oppenheim saw many large stones thus extracted with a happy result. The Turks, he observes, are much less liable than the Franks to suffer bad consequences from wounds or operations; this was strongly exemplified among the wounded of the Russian and Turkish armies; the habitual abstinence of the latter from spirituous liquors, and their moderation in the use of animal food, may contribute, he thinks, to render the consequences of inflammation less violent and dangerous. Diseases of the testicles and their appendages are very frequent, and Dr Oppenheim justly attributes this to the narrow curved shape of the Turkish saddles, and their peculiar mode of riding, in consequence of which, the testicles receive a great shock each time the horse is suddenly checked; a practice they are very fond of in shewing off their horsemanship.

Observations on Eunuchs.—Dr Oppenheim's observations on eunuchs are interesting.

"Jealousy, the natural effect of polygamy, has rendered the Mahommedans extremely watchful of their wives, and has multiplied the number of eunuchs. The sufferers are brought in their youth from Africa, where they are purchased as slaves in Sennar and Darfour, and are delivered into the hands of the Coptic monks in Egypt, who almost exclusively cultivate this branch of national industry. The occupation is detested by the Egyptians, but the government, nevertheless, protect those who are engaged in it, on account of the profits derived from so lucrative a trade. The Pascha of Egypt has conferred certain privileges and immunities on the village *Zawyet el Deyr*, near *Saout*, because its inhabitants (who call themselves Christians!) have obtained great celebrity in this occupation. The number annually thus mutilated is certainly considerable, but it falls far short of the estimate of Tavernier, who says that, in 1659, twenty-two thousand eunuchs were sold in one province alone. Many, no doubt, die of the operation; the age generally selected as the safest for its performance is from six to seven. The operator encloses the testicles and scrotum with a tight ligature, and then cuts them off at one stroke with a sharp razor. The actual cautery, or styptic powders, the composition of which is a secret, are used to restrain the bleeding. Although the best-looking boys are selected, yet they never grow up handsome, and the adult eunuch may always be distinguished, not merely by his childish piercing voice and want of beard, but by a certain expression of premature old age, together with hollow eyes and prominent cheek bones. The opinion entertained by some, that eunuchs, being deprived of enjoyment themselves, hate the rest of mankind, and are cruel and unfeeling, I must positively deny, for I have known many of a kind and most benevolent disposition, nay I have watched with surprise how contented and happy they seemed to be. It is true, when provoked, they are hasty and revengeful, but what African is not? The number sold annually at Constantinople is about 300; they generally cost about 20,000 Turkish piastres a piece, while a common male slave is sold for one or two thousand."

Dr Oppenheim had several opportunities of witnessing the performance of circumcision in Turkey; in a surgical point of view it offers nothing of interest, and therefore, at present, it is sufficient to remark, that it is not performed until the sixth year, and the day of circumcision is celebrated by great feasts, and by presents from every member of the family. Dr Oppenheim's account of the pomp and ceremony observed on such occasions in the families of the great is very amusing. It may be well to mention, that the inner layer of the prepuce is not cut so short as the outer, and consequently it still affords a covering to part of the glans. In many, so small a piece of the fore-skin is cut off, that it is not easy to say, when the person grows up, whether he has or has not been circumcised,—an assertion illustrated by the fact, that Dr Oppenheim was twice obliged to perform the operation for phymosis on Moslems.

Ophthalmia is very common in Egypt, but not in other parts of Turkey. The native surgeons are of opinion, that the cataract is produced by some foreign body, which falls from the head into the eye: they are very expert in performing the operation of depression with a needle, which they insert through the cornea. Dr Oppenheim created great astonishment by the effects of an electrical machine, which he occasionally used in cases of amaurosis.

The blind are provided for to a considerable extent in Turkey, being the only persons allowed to ascend the minarets or towers of the mosques, for the purpose of proclaiming the times of prayer, five times a day. As these towers command an extensive view of the tops of houses, where the women often air themselves and bathe, none but the blind are permitted to perform this office.

The Dumb great favourites.—The dumb are favourites with the great, and are constantly employed as valets to wait on them, in the most confidential manner; this partiality is founded on the belief that they can tell no tales. But Dr Oppenheim testifies, that they do not always possess this good quality; for in the campaign against the Albanians, most important intelligence was received, through the means of a dumb spy who waited on one of the revolted chiefs. ROBERT J. GRAVES.

OBSERVATIONS ON THE STRUCTURE OF RECENT AND FOSSIL CONIFERÆ*. *By* WILLIAM NICOL, *Esq. Lecturer on Natural Philosophy, &c. Communicated by the Author.* With three Plates.

THE structure of recent and fossil Coniferæ has lately become an object of considerable interest on the continent as well as in this country. At different intervals during the last three years, I have paid some attention to the subject, and, from the investigations I have made, it would appear that much still remains to be done. There are several circumstances in the structure both of recent and fossil coniferæ, which seem entirely to have escaped the notice of those who have lately written on the subject, and which, had they been known, would have saved the authors the trouble of forming from a few hand specimens of fossil wood, genera said to be totally different from any of the recent Coniferæ. To pronounce with certainty whether a fossil conifera be essentially different from any known individual of the recent kind, it would be requisite to have a thorough knowledge of the structure at least of all the different tribes of recent coniferæ; and yet several distinct fossil genera have been indicated by a person who has examined, and that too very superficially, only three slices of three recent pines, differing not essentially from one another. Before I attempt to indicate the points of agreement and disagreement between certain fossil and recent coniferæ, it seems right to premise a few observations relating to the general structure of the latter, as seen both in the transverse and longitudinal sections.

In the *transverse section*, by far the greater part of the Coniferæ present distinct annual layers. These often vary considerably in their relative breadths. In general, however, they are broadest towards the centre, and gradually diminish in that respect towards the surface. This may be seen in many of the pine tribe. In a planted Pinus sylvestris, for instance, from Invercauld, in Aberdeenshire, eighty years old, from the pith to the distance of one inch there were only four annual layers. At half the distance, between the pith and the surface, there were ten layers in an inch, and near the surface there were fifteen layers in an inch. In a native tree of the same species, from the same quarter, 160 years old, the diameter of which was exactly the same as the other, the relative breadths of the layers were remarkably different. The broadest layer in this tree exceeded not the tenth of an inch; whereas, in the other, the broadest was nearly three tenths. From the pith to the distance of one inch, there were twenty-six layers. From the age of 40 to 52 years namely, twelve layers occupied an inch. From the age of 70 to 86 years namely, sixteen layers occupied an inch; from the age of 105 to that of 125 namely, twenty layers occupied an inch; from the age of 130 to that of 160 namely, thirty layers occupied an inch. From these measurements it will appear that, throughout the whole semi-diameter, the broadest layers of the native tree were scarcely double the breadth of the narrowest; whilst, in the planted tree, some of the broadest layers were upwards of four times the breadth of the narrowest. The diameter of these two trees being the same, the growth of the native must have been slower and much more uniform than that of the other; and this uniformity of growth is, in all probability, a principal cause of the well known superior strength of the native pines in the Highlands of Scotland. On observing the differences between these two trees, I was naturally led to examine some other individuals of the same kinds, and found the same conformation in the whole of them.

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* Read before the Wernerian Society on Saturday, 14th December 1833.

A striking illustration of the superior strength of the native Scotch firs occurred during the erection of a bridge, which, a few years ago, was thrown over the river Don, in the vicinity of Aberdeen. In consequence of the softness of the ground, it was found necessary to build the bridge on piles thirty feet long. Logs of the best Memel timber were first employed, but it was soon found that their tenacity was insufficient to resist the force necessary to drive them to the requisite depth, although their upper extremities were strongly guarded with hoops of iron. Recourse was then had to the native pines of the forests of Braemar, and these withstood the utmost efforts of the driving machine.

I have stated that the annual layers are generally broader near the pith than towards the surface, but, in certain species, as Pinus Larix, Juniperus communis, &c., even when the stem is nearly cylindrical, the breadth of some layers is many times greater on one side of the tree than it is on the opposite side. It sometimes happens, too, that a layer of considerable breadth on one side becomes so narrow on the other side as entirely to vanish, and some layers frequently assume very different breadths in different parts of their circuit. The Juniperus communis often presents these undulations in an elegant manner.

The annual layers of the Coniferæ, wherever they occur, are always separated from each other by distinct lines of demarcation, and, as far as my observations have extended, they occur in all the true pines, yews, junipers, cypresses, and thujæ; but, in the tribe of Araucarias, they are entirely wanting. I have seen, it is true, only two of the four known species of Araucaria, namely, the Braziliana and Cunninghami. These have certainly no distinct annual layers, and it is highly probable that the same character extends to the other two species. The Brazilian specimen in my possession, which grew in this country, is a portion of a very young stem; but the section of the other, for which I am indebted to the kindness of Dr Greville, is from Moreton Bay in New Holland, and is nearly fourteen inches in diameter. Irregular zones, displaying a slight diversity of colour, are to be seen throughout the whole surface, but there is no appearance of the distinct bounding lines that characterise the annual layers of all the other tribes of Coniferæ. As I have had no opportunity of examining either of the two species of Dammara, I am unable to say whether they have annual layers or not. I shall, therefore, not indulge in conjecture, but express a wish that attention may be given to the want of annual layers in the tribe of Araucaria, as it will enable us to explain why annual layers are not to be seen in some fossil Coniferæ.

Besides the want of annual layers, there are other distinguishing characters to be seen in the Araucariæ, to which I shall presently have occasion to allude, and, with regard to the annual layers, it may be right to subjoin that they occur in pines growing in the warm as well as in the temperate regions. I have seen them in various species from the East Indies, as in the longleaved pine called Cheir, from Fora, opposite Gospoor,—the fir from Nepaul, and in the Baltee fir.

When a transverse section of any of the Coniferæ of the requisite thinness is viewed with a lens of the requisite magnifying power, a reticulated texture is seen, which bears a considerable similitude throughout the whole species. Rays or lines of different degrees of thickness appear extending from the pith to the surface. The boldest of these often preserve a rectilineal course, but some of the more slender kind have, in some places, a kind of zig-zag form. The whole are connected by concentric lines or partitions, occasionally also varying a little in thickness. The meshes or network resulting, must consequently sometimes vary a little in form. When two or more radial lines are equidistant and nearly of equal degrees of thickness, the concentric lines cross them at nearly right angles; but, as the distance between the concentric lines is generally greater than the distance between the radial lines, at least near the inner side of each layer, the meshes or openings are a little elongated in the direction of the radii. Towards the middle of the layers the meshes become nearly equilateral, and towards the outer edge of the layers the concentric partitions approximate each other, when the meshes become elongated in a direction perpendicular to the radii. This description will, in general, be found to apply to many pines, at least when they have attained a slow and consequent uniformity of growth. Near the pith where the annual layers are generally broad, and, of course, the process of vegetation had been rapid, the meshes often assume a considerable diversity of form. They are sometimes pentagonal, sometimes hexagonal, and sometimes of no very definite form. In some pines, the partitions at their points of intersection increase a little in breadth, especially when the slice is of a certain thickness, which gives rise to a subhexagonal form, and which is sometimes more frequent in one tree than in another, even of the same species. The quadrangular form of the meshes, I have frequently observed to be much more conspicuous in some of the large American than in several of the European pines of the same species. In a Pinus Abies, for example, of large dimensions, from the forest of Braemar, in Aberdeenshire, the irregular was much more frequent than the regular form of the meshes, throughout the whole transverse section; whereas, in a tree of the same species, from the British Colonies in North America, the regular quadrangular form of the meshes was as predominant as in any species of the pine tribe I have ever examined. To acquire a thorough knowledge of the structure of any kind of wood, a single section of small dimensions will by no means suffice; and some of those who have lately attempted to instruct the world with regard to the difference between fossil and recent coniferæ, have done themselves no credit, and the world some injury, by drawing conclusions from a single, and that too imperfectly cut, section.

In some of the junipers as well as in the Thujæ, the quadrangular form of the meshes very generally prevails; but perhaps the greatest irregularity in this respect is to be seen in the Araucarias. In this tribe the meshes are seldom much larger in one direction than another; but even when they approach the quadrangular form, the partitions are often less rectilinear than in many of the true pines, and the meshes from the centre to the surface are more uniform in size than in any of the pines, the process of vegetation having sustained no regular annual interruptions.

Besides these differences in the form, &c. of the meshes, there is a peculiarity in the Araucarias which I have not observed in any one species of the coniferous family. The peculiarity consists in an unusual weakness or want of tenacity in the concentric partitions. This weakness is such that with every care, and with the keenest edged instrument, it is scarcely possible to cut a transverse section of any considerable size, without the medullary rays separating from each other. To procure even a very small slice with the radial partitions in a state of adhesion, it is necessary to place the edge of the instrument parallel to the medullary rays, and to press it forward in that direction; for if it be made to move in a diagonal direction, which in general is the best direction for making a very fine slice, scarcely two rays will be found to adhere to each other. Hence we may infer, that the timber of the Araucarias, if the whole species be similar to that from Moreton Bay, must be greatly inferior for various architectural purposes to that of many of the true pines.

On comparing the relative breadth of the partitions with the size of the meshes or openings, in a transverse section of different species of Coniferæ, we find a very considerable difference. In some, as the Pinus strobus, Pinus canadensis, &c., the ligneous substance composing the partitions occupies a much smaller space than that of the intermediate meshes. In others, as the Taxus baccata, the ligneous substance composing the partitions occupies a larger space than that of the meshes, and the yew is well known to be greatly superior to any of the pines in strength or tenacity. The differences in question may be seen by inspecting the first and fourth figures of Plate II. I may here observe, that the whole of the sections figured in all the plates were drawn of the sizes as they appeared to the eye, when viewed with a double convex garnet lens of nearly a fortieth of an inch radius.

The transverse sections of Coniferæ present appearances of so much similitude, that it is not very easy in every case to distinguish one species from another, especially in the pine tribe; but the finer texture might enable an eye familiar with the subject to distinguish the Junipers and Thujæ from the Pines, and the irregular form of the meshes in Salisburia and Araucaria might enable one to distinguish them not only from the pines, but also from the Junipers and Thujæ.

Besides the general reticulated texture as seen in the transverse section, occasional circular openings of larger dimensions than any of the meshes appear in many of the true pines. They often occur in Pinus strobus, P. sylvestris, P. abies, and P. Larix; but I have not observed them in Pinus picea, nor in P. canadensis, nor in P. cedrus, nor have I ever seen them in the Junipers, the Thujæ, the Cypresses, the Salisburia, or in the Araucarias. They are called Lacunæ from the circumstance of their appearing empty, especially as seen in very thin transverse sections. They are longitudinal tubes, and are completely filled in their whole length with slender membranous substances, which seem to lie in different directions. When a longitudinal section passes through the centre of one of these tubes parallel to its axis, the contained membranes are easily seen.

Longitudinal Sections.—The elegance of the reticulated texture displayed in the transverse section of many of the Coniferæ, must arrest the attention of every observer; but it is in the *longitudinal section* parallel to a radius, that we are to look for the most beautiful mechanism the Coniferæ display. To cut such a section, however, in a proper manner, is often attended with some little difficulty, arising from the different degrees of hardness on the opposite sides of each annual layer. The section must be parallel to a radius. It must be nearly of an uniform thickness, and neither too thick nor too thin. If too thick, the structure of the parts does not come into view, and if too thin, only a shadow of the whole remains.

Such a section, when seen with a proper magnifying lens, presents the longitudinal vessels with their bounding partitions well defined. The vessels are generally widest at the inner side of each annual layer, and gradually diminish in that respect towards the outer edge. Near the latter they are often so narrow that the partitions almost touch one another. In some parts the vessels are often rectilinear to a considerable extent, and in other parts they often assume a curvilinear form, and even inosculate or cross one another. At irregular intervals the vessels are often traversed at right angles by bundles of straight lines more or less numerous, and these may often be seen stretching across several annual layers without interruption.

In every species of the family of Coniferæ, whether they contain annual layers or not, the longitudinal vessels, as seen in the longitudinal section parallel to a radius, are in some places apparently empty, but in other places they contain groups of circular objects more or less numerous. These objects, or *discs* as they may be called, are of different dimensions in different species, and in every case where there are annual layers they are always of the greatest diameter at the inner side of each layer. They diminish in size towards the outer side, and when the vessels become very narrow they generally disappear. The discs are often pretty regular circles, especially when they are situated at a certain distance from each other, but they sometimes become slightly elliptical especially when near one another, and in such cases the transverse axis is nearly perpendicular to the longitudinal vessels. When the discs are very near each other, the contiguous surfaces often become rectilinear, and when they are large, they often project into the partitions, and give to their edges an undulated form.

These discs are composed of a considerable number of concentric lines, which are always very much crowded together, especially near their circumferences. A space at the centre often contains one circular curve surrounded with a broadish curve; which in some kinds of wood has an elliptical form. The curves or lines towards the centre are often not exactly in the same plane as those towards the outer edge, their focal distances being somewhat different. But in some pines the whole disc displays a pretty uniform distribution of curves from the circumference to the centre. It is impossible in words to convey to the mind any idea of the beautiful appearance of these discs, when duly illuminated and viewed with a single lens of the requisite magnifying power. Fig. 2, Pl. II., will convey some idea of their structure as seen in the Pinus strobus from North America; but the minute assemblages of prismatic colours as seen in the microscope, cannot be represented either by the pencil or the tool of the engraver.

In some of the Coniferæ the discs are arranged in the vessels in single rows only, in others they are arranged in double as well as in single rows. In Pinus sylvestris, in P. Abies as well as in the Junipers, Thujas, and Cypresses, I have not observed more than single rows of discs in each vessel; but in Pinus strobus, P. canadensis, Taxodium disticha, and in the Araucarias from New Holland and Brazil, double as well as single rows are often to be seen. When double rows of discs occur in one vessel in Pinus strobus, P. canadensis, or in Taxodium disticha, the discs of both rows are placed in such a manner that a line drawn from the centre to the surface of the wood would pass through the centres of each, or in other words, the discs in one vessel are placed side by side, and never alternate with each other. This arrangement will at once be seen by inspecting the figures in Plates II. and III., and it may be right to bear it in remembrance, as a very different arrangement will be shown to exist in the double rows of discs in the Araucarias. There is another reason for attending to the circumstance, namely, the bold assertion in Mr Witham's Treatise on the Structure of Fossil Trees, that in the vessels of the recent Coniferæ, the discs or areolæ, as they are called, occur in *single* rows only.

I have stated that in the Taxodium disticha, there are double as well as single rows of discs similar to those occurring in Pinus strobus and P. canadensis; and this is strictly true with regard to one portion only of the piece of wood I examined. The other portion of the wood presents appearances so totally different, that no one could suppose it possible that both existed in the same piece of timber. Whether the peculiarity be a general or only a casual circumstance I am unable to determine, having hitherto had no opportunity of examining any other piece of the same kind of wood. That which I examined grew in the vicinity of London; its mean diameter is about four inches. The pith is considerably nearer one end of the longest diameter than the other. About two-thirds of the transverse section is of a darkish cream colour. A space about an inch in diameter, with the pith in its centre, is of a chestnut-brown; the remainder is a shade lighter, or a darkish wood-brown. Now the whole of the pale portion of the wood presents in the longitudinal section, parallel to a radius, double and single rows of discs similar to those occurring in Pinus strobus, as shewn in Fig. 2. Plate III.; but throughout the whole of the darker part, the discs, which occur in single rows only, are so very obscure, that the concentric circles can scarcely be seen. Fewer patches of the discs occur in this than in the paler part; and even where they do occur, the individual discs are always placed at a greater distance from each other. Besides the dissimilitude of the discs, the longitudinal vessels throughout the dark portion of the wood contain numerous lines or fibres, crossing each other nearly at right angles, but inclined to the partitions at an angle of about 45°, of which there is not the slightest trace in the pale part of the wood. These lines have often the appearance of a St Andrew's cross, especially when they pass through the discs. As sections both of the pale and dark parts of this singular piece of wood are figured in Plate III., a better idea of the respective differences will be had by inspecting the figures, than can be given in words, but both fall greatly short of the reality, as seen in the sections themselves, when the eye is aided by a powerful lens.

The concentric longitudinal section of the dark part of the wood, presents in the vessels decussating fibres, similar to those occurring in the section parallel to a radius; and both in the dark and pale part of the wood, the partitions of the vessels in the concentric section have expansions or openings similar to each other.

A difference is also observable in the transverse sections of both parts of this wood. In the pale part, the meshes are considerably larger than in the dark part, and the dark part is considerably harder and firmer than the pale part. It would be interesting to see whether in the large and full grown trees in America, the Taxodium disticha presents appearances similar to those above described. That tree being a native of some of the more southern states, is never, as far as I know, imported into this country. It is, therefore, to be hoped, that some of the many scientific gentlemen in America will examine it in both its sides, and favour the world with the result of the investigation.

If I may judge from the few longitudinal sections I have cut, the Taxus baccata contains discs arranged in single rows only. They are smaller, and even more obscure, than those situated in the dark side of the Taxodium disticha. They scarcely exhibit distinct concentric curves, but this may arise from the difficulty of cutting slices sufficiently thin. As in the other Coniferæ, they occur in groups, but the individual discs are always detached from each other; and, as in the other Coniferæ, there are many parts of a section where no disc is to be seen. The longitudinal vessels are so very narrow, that the partitions nearly touch one another, at least in certain parts. The partitions consist of extremely minute parallel lines, and in the vessels there are many distinct transverse lines, somewhat resembling those in the dark part of Taxodium disticha. In the yew, however, these lines are placed in a direction nearly perpendicular to the sides of the partitions. They scarcely ever cross one another, and when they do meet, it is only at their extremities, where they give rise to an appearance resembling that of a very acute letter V placed in a horizontal position thus >. In many places the lines are quite parallel. In the concentric section of Taxus baccata, the partitions of the vessels occasionally present lenticular expansions, in which there are two, three, or four subelliptical openings, somewhat resembling those in the darker part of Taxodium disticha. The appearances presented by the different sections of Taxus baccata are figured in the under part of Plate III.

The Salisburia adiantifolia, which differs so widely in the form of its leaves from any of the pines, also presents some differences in the structure of its wood. The annual layers are not so distinctly defined in their boundaries as in most of the pines, and, though the general form of the meshes is quadrangular, yet they are often very irregular and of different sizes. In the longitudinal sections, the vessels, though varying a little in width, are more rectilinear than in most of the pines, they seldom or never running into each other. The discs do not occur so much in patches as in most of the pines, they being more extensively and uniformly distributed. In the small piece of wood I had to operate on, they occurred in single rows only. The partitions of both longitudinal sections, present the fibrous structure very distinctly, and in those of the concentric section numerous short expansions occur, seldom containing more than two openings. I could have wished to present engravings of the sections of this wood, but there has not been time to get the drawings ready.

When proper sections of the Araucariæ are duly examined, we find appearances which differ even more widely than those of the Salisburia from the rest of the Coniferæ. The want of regular annual layers is a sufficient characteristic distinction in the transverse section, and the form and arrangement of the discs is equally distinctive in a longitudinal section. In the longitudinal section of the Araucarias, parallel to a radius, groups of discs, arranged both in single and double rows, are often to be seen. In some parts the groups are more numerous than in others, and a solitary row, consisting of a few discs only, may often be seen where no groups occur. There is a peculiarity in the single rows of discs which alone would distinguish the Araucariæ from any other tribe of Coniferæ. In the Araucarian rows, the discs are always placed near one another and at equal distances, and each row, whatever be its extent, terminates abruptly at both its extremities. In some of the single rows the discs, though near one another, are quite distinct and pretty regular circles; but it sometimes happens that the discs are as if squeezed together, when they become slightly elliptical.

In the Araucarias the discs, particularly those in the single rows, are larger and much more distinct than in the yew, but they are smaller and more uniform in size than in any of the true pines. The concentric circles of which they consist are often very distinct, and, when duly magnified, they present a very beautiful appearance; but by far the most striking feature of the Araucarian discs is to be seen in the double rows. In the pines the discs in all the double rows, as already observed, are always placed side by side; whereas, in the Araucarias, the discs in the double rows always alternate with each other. This alternation of the discs in the double rows, has modified in a very striking manner the contiguous parts of the circumference of each disc. Instead of being circular, the approximating parts have become rectilinear, which has rendered each disc partially polygonal, and wherever the discs in one row are equally near one another as those in the two rows, their contiguous parts are often also distinctly rectilinear, so that each disc is in four-sixths of its circumference rectilinear, two-sixths only retaining the circular form. The discs in the double rows of the Araucariæ are, therefore, partly circular and partly polygonal, the polygonal part consisting of four distinct sides.

The polygonal form which these discs have partially assumed, affords a striking instance of the effects of opposite forces mutually acting against each other in the process of vegetation; and there can be very little doubt, that were triple or quadruple rows of discs to occur in any tree arranged in the manner of those in the double rows of the Araucaria, we should have a series of regular hexagons, more distinctly defined than any of those hitherto seen in the fossil Coniferæ. Whether such an arrangement exists in any living tree cannot be determined, until the structure of all the Coniferæ be examined. That a structure similar to that above described exists in all the Araucariæ is highly probable; but the Dammara and Callitris remain for investigation, and other tribes may possibly be found, that have hitherto escaped the research of the botanist.

There is nothing very striking to be seen in the concentric section of the Araucaria. The expansions of the partitions are if any thing rather larger than in some other Coniferæ. They have a cylindrical form, and contain several elliptical openings which are often confluent. The partitions of the vessels in this, as in the radial section, present a very distinct fibrous structure. A reference to Plate IV. will, however, convey a better idea of the peculiarities displayed in the different sections than can be given in words. Fig. 1. represents the transverse section; Fig. 2. the arrangement of the double rows of discs with their polygonal boundaries; Fig. 3. shows the arrangement of the single row of discs; and Fig. 4. is a representation of the concentric section.

Although I have already mentioned the circumstance, it may not be improper here to repeat, that the whole of the sections figured in the three plates, are of the relative sizes, as seen by a double convex garnet lens of about one-fortieth of an inch radius. They may, therefore, be considered as magnified about four hundred times; and to give some idea of the minuteness of some of the parts represented, I may mention, that in one row a twentieth of an inch long, I have counted not less than fifty discs. The diameter of each could not, therefore, have exceeded the thousandth part of an inch. But the smallest of these discs are of an enormous size when compared with the fibres of the partitions bounding the vessels in which they occur. By using the same magnifying power, in making drawings of this kind, we are enabled to judge correctly with regard to the relative dimensions of the corresponding parts. In the sections of some kinds of wood, I may add, the structure of the disc is to be seen to great advantage when magnified about three hundred times, and, with very few exceptions, the objects are better seen by artificial light, as that of a candle, than by that of day.

The whole of the drawings in the three plates were made by Mr Scott, Surgeon in the East India Company's Service. Every attention was bestowed to render them accurate representations, and gentlemen, who have compared them with the sections from which they were taken, have expressed the same opinion of their general accuracy.

Having thus given a brief description of the structure of several individuals of the family of recent Coniferæ, I shall now subjoin a few observations on the structure of those which occur in the earth in a fossil state. Some of the fossil Coniferæ have distinct annual layers, others have none. The first kind more frequently occur than the second, but the second have been met with in various places. In the lias formation in the vicinity of Whitby, I found a small specimen, which, although it displayed the coniferous texture in a very perfect state, showed no indication of distinct annual layers. This specimen was entirely siliceous. Similar masses of considerable magnitude, also siliceous, have been found in the Newcastle coal-formation; and in the sandstone quarry of Craigleith, several trunks of large dimensions, first pointed out to naturalists by Professor Jameson, have been found, without any appearance of annual layers *. These, however, are chiefly composed of carbonate of lime, blended with a portion of iron and carbonaceous matter. The trunk which was discovered in that quarry in October last (1833), is perhaps the most magnificent specimen of a coniferous petrifaction that has ever been seen. It is nearly round, and nearly three feet in diameter. In its position it declines from the vertical about 33°. Fourteen feet of it have already been laid bare, and every precaution is taken to preserve it entire until it be wholly dug out. When that happens, it will probably soon after be deposited in some one of the scientific institutions of Edinburgh. When this trunk was first observed, some fragments were detached from its upper extremity. From the most solid of these I have cut several sections, both transverse and longitudinal, with a view to examine its structure. One of these specimens measures about twenty square inches. Throughout the greatest part of its surface, the coniferous texture exists in a very perfect state of preservation, but towards one of its extremities the vessels are very much contorted, and in one small part completely obliterated.

The colour of this tree is in the transverse fracture greyish-black; in the longitudinal fracture brownish-black. Throughout

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* The largest of these is figured in Mr Witham's work.

that part where the structure is most perfect, there are many curvilinear lines of a darker colour than the general shade, and a similar shade in the form of belts often pervades those parts where the structure is most deranged. Small veins of white calcareous spar are often to be seen throughout the whole mass.

In the longitudinal section parallel to a radius of the present Craigleith tree, the vessels in general are very much distorted, and the whole texture very much obscured; but wherever the vessels appear with any distinctness, they often contain discs in double, triple, and even quadruple rows. The discs are always very obscurely seen, so much so, that in many parts they are barely discernible. In several parts, however, they are sufficiently distinct to leave no doubt as to their hexagonal form. The vessels in the concentric section are even more distorted than in the other. The partitions, however, in some parts, afford traces of expansions similar to those in the Araucariæ; and with the exception of the triple and quadruple rows of discs, the Craigleith tree very much resembles, in its general characters, the Araucaria from Moreton Bay.

The siliceous specimen above mentioned from the vicinity of Whitby, which is destitute of annual layers, affords no trace of discs in the longitudinal section parallel to a radius, but their absence now is no proof that they did not exist in the wood when in a recent state. In the fossil Coniferæ, the discs in the longitudinal vessels are in general very much obscured, and frequently disappear even where the reticulated texture and partitions of the longitudinal vessels are perfectly distinct.

A specimen, for example, which I found on the beach under the cliff of the upper lias in the vicinity of Whitby, which, in the transverse section, displays not only distinct annual layers, but also the most perfect coniferous reticulations, affords but very few traces of discs in the longitudinal section. The longitudinal section I allude to is two inches long in the direction of the radii, and nearly one inch in the longitudinal direction, and it is only in a small portion at one end that the discs can be seen. They seem to be as large as those in many of the recent pines, and like them they are either circular or slightly elliptical. They chiefly occur in single rows, but some few traces of double rows are observable, and in that case the discs are placed side by side, in the same manner as they are placed in the vessels of all the true pines. Were this slice divided into two unequal portions, one of them would have been styled a Peuce and the other a Pitus or Pinites; and parts of it would have furnished materials for the genus Anabathra of Witham, as they frequently present between the partitions of the longitudinal vessels distinct transverse lines, somewhat resembling the steps of a ladder*. In the section perpendicular to a radius, for in fossils it is not easy to produce a truly concentric one, the partitions of the longitudinal vessels are often very indistinct, but some of them present cylindrical or elliptical expansions, containing a single row of many roundish openings.

In another very fine specimen of fossil wood which I also found in the vicinity of Whitby, the transverse section shows well-defined annual layers, displaying the most perfect coniferous reticulations. The longitudinal section parallel to a radius is in some parts rather imperfect, but in many parts the partitions are pretty well defined. Between these, both single and double rows of very obscure discs are occasionally to be seen. The greatest part of the section, however, is destitute of discs. The discs in the single rows are apparently either circular or elliptical, but those in the double rows are decidedly polygonal, and the discs in one row alternate with the discs in the other row. In this circumstance of distinct annual layers, this fossil agrees with the recent pines, and in the form and arrangement of the discs with the Araucarias.

Transverse sections of this fossil are figured in both the editions of Mr Witham's work, and they are the only ones acknowledged to have been furnished by me, although, with a single exception, I furnished the whole of the sections from the lias formation figured in the first edition. Some sections of the fossil in question have been figured on a larger scale in the 15th plate of the second edition, under the name of Peuce Lindleiana. The second figure is said to represent a portion of a longitudinal section parallel to a medullary ray; but it so little resembles the similar sections I have made, that I cannot but suspect it to be a representation of something else, more especially as I gave the lapidary permission to take off for Mr Witham one transverse section only. The first two single rows of discs in the figure to the left, are represented as each displaying two bold concentric circles; whereas in the sections I have made, the discs are altogether so very obscure, that they can scarcely be seen. There is only one row of double discs in the figure, and the discs are irregular circles; whereas, in my sections, the double rows are by far the most frequent, and the discs are all decidedly polygons. In page 61 of the 2d edition, we are told that "the cellules are very regular, and present generally one, sometimes two, series of circular glands or areolæ, which are precisely similar to those of recent Coniferæ; and that the only difference is, that, in the latter, the areolæ are always in single series, whereas here they are sometimes in double series, as in the fossil genus Pinites." By stating that the areolæ, or discs, as I call them, are precisely similar to those of recent Coniferæ, it is evident that the writer of the book was not only ignorant of the fact that the areolæ are circular in the Pines, and partly polygonal in the Araucarias, but also that double rows of discs are of frequent occurrence in the recent as well as in the fossil Coniferæ. The genus Pinites must therefore be rejected, if better characters cannot be found for it. Perhaps it may be thought that I have been too prolix on this part of the subject, but I consider it necessary, to guard the scientific world against placing too much reliance on a work, containing so many inaccuracies.

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* *Vid.* Witham on Fossil Vegetables.

In the lias formation in the vicinity of Whitby, we have at least three fossil Coniferæ, differing in their characters from one another. The first, which is siliceous, resembles the Araucarias, in having no distinct annual layers; the second resembles the Pines, in having annual layers, and also in having similar discs similarly arranged; and the third, resembling the Pines in having annual layers, but differing from them in having polygonal discs arranged in double rows, the discs in one row alternating with those in the other; yet the writer of the book alluded to states, that all the longitudinal sections he has seen of the Whitby lias fossils are so similar in their characters, that he can only say of them that they all belong to the genus Peuce. If so, the genus Peuce must be a very heterogeneous one.

I might here point out the characters of some other fossil

Coniferæ which I found in the lias deposit of Whitby, sections of which are also figured in Witham's Treatise, but having already, I fear, exceeded the limits of a communication of this kind, I shall leave that district, and add little more than the description of a fossil Conifera, which I found among the debris of the prismatic columns of porphyritic pitchstone constituting the Scuir of the island of Eigg. Sections of this specimen are also figured in *the* book, but the writer states erroneously, that the specimen was found, not among the debris of the pitchstone, but that of the lias rocks in the vicinity of the Scuir.

The Eigg fossil in the transverse section presents annual layers well defined, and displays the coniferous reticulated texture in great perfection throughout the greatest part of the surface. In some few parts towards the outer edge, the texture is very much distorted, and in one part nearly obliterated. The obliterated part is replaced by small translucent circular portions of sparry matter, which in some parts are distinct, but in other parts completely confluent. Portions of this small circle of spar casually occur in the perfectly reticulated part, which some have considered as lacunæ; but those towards the outer side not only become larger, but gradually approximate each other, and at last entirely obliterate the meshes. That the whole are not lacunæ, may also be inferred from this circumstance, namely, that in the centres of some of the circles, portions of the reticulated texture may be distinctly seen.

Some dislocations have taken place in this specimen, without the parts having been separated from each other. Of these, some are in the direction of the radii, others in a concentric direction. The former are discernible merely in consequence of the edges of the layers on one side of the slip being opposite the middle of the layers on the other side, and the latter in consequence of the medullary rays in one layer not passing through the slip into the adjoining layer.

In the longitudinal section parallel to a radius, this fossil shows no indication of discs of any kind. The partitions of the vessels are very much crowded together, greatly distorted, and the vessels furnish nothing of a characteristic kind.

The authors of the "Fossil Flora *," have called the Eigg fossil Pinites Eggensis, and assert that it differs essentially from any of the coal Coniferæ. Their observations must have been confined to the few fossil trees that have been found in the vicinity of Newcastle. These, as far as I have seen, are all destitute of annual layers, and in that respect they certainly differ from the Eigg fossil; but I have examined specimens, in the possession of Professor Jameson, from the coal formation of New Holland, so closely resembling that from Eigg, that few could have distinguished the one from the other.

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* Lindley and Hutton's periodical work on Fossil Plants.

In Professor Jameson's collection there are specimens from the coal formation of Nova Scotia in America, one of which is a fossil Conifera displaying all the characters of the most perfect recent American pines. In the transverse section, the annual layers are well defined, the reticulated texture large and perfect, and in the longitudinal section parallel to a radius, discs occur both in single and double rows. These as usual are in some parts very obscure, but in other parts they are very distinct. In size the discs are about as large as those in Pinus canadensis. They are circular, and some of them display at the circumference two concentric rings, and one ring near the centre. In the double rows, as in the recent pines, the discs are placed side by side; and indeed, in all its characters, this fossil bears a greater resemblance to some of the recent pines than any thing of the kind that has hitherto fallen into my hands.

Many other particulars relating to fossil Coniferæ might have been adduced, but these I reserve for some future communication, and shall at present rest satisfied with briefly stating, as a general conclusion from what I have hitherto observed, *that all the fossils retaining the ligneous structure in the coal and lias formations, are of coniferous origin, and that, with one exception, those in the tertiary formations are either monocotyledons or dicotyledons.* I have examined upwards of a hundred specimens of fossil wood, from the tertiary formation of the island of Antigua, and also several from a similar formation in the island of Java, in the possession of Professor Jameson, without finding a single Conifera among them. The specimens from Antigua were chiefly dicotyledons, the rest monocotyledons. Those from Java were all dicotyledons, and the exception to this distribution occurs in the tertiary formation of the isle of

Sheppy, in the county of Kent. In the Royal Museum of the University, there are two specimens from that locality which are unequivocal Coniferæ, and apparently both of one species.

The above conclusion I have long ago made known to many scientific gentlemen in this country, and also to several on the Continent; and although a statement of it was read to this Society, and printed in the 27th number of Professor Jameson's Philosophical Journal, yet, ever since the publication of Mr Witham's first work,* an opinion has generally prevailed that the conclusion was the result of Mr Witham's investigation. This opinion did not arise from any direct statement to that effect, but from the author of the work not expressing in *direct terms* that the conclusion had been pointed out to him by me. I attach no merit to myself for the *discovery I made*, since any one might have made it with even the half the labour I bestowed, but justice requires the correction of an error already too far and widely spread.

The method of investigating the structure of Coniferæ by the characters displayed in the longitudinal sections, has been much vaunted in the second edition of the "*Observations on Fossil Vegetables*," as a new and important discovery. This method I had often employed, but I must say that it was Professor Lindley who first published the advantages resulting from it.

There is another circumstance which I certainly should not have thought it worth while to notice, had it not found its way into several scientific works in this country and abroad, which is the prevalence of an opinion that *my method* of preparing thin slices of fossil wood for microscopic observations, is also the contrivance of Mr Witham. This opinion has chiefly arisen from the repeated assertions of the joint Editor of a work on Fossil Plants, in spite of what is stated by Mr Witham himself, who says in the first edition of the "Observations on Fossil Vegetables," viz. *that he is indebted to me for the process.*

Not only my method of preparing thin slices, but that of examining the organic structure of fossil woods, by reducing them to thin translucent slices, has been ascribed by some unwise friends to Mr Witham, although that gentleman has no more claim to the latter than I have. It was first employed in this quarter by Mr Sanderson, the lapidary *.

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* Observations on Fossil Vegetables by H. Witham, Esq. F. R. S. &c. 1831.

The manner in which he (Mr Sanderson) prepared his sections was attended both with difficulty and uncertainty, and was inapplicable to the making of large sections of fragile fossils. He first ground a surface flat, and then with lapidary's cement fixed that surface to a block of wood. The fossil was afterwards ground down until it was thought thin enough. When that was done it was difficult to detach it from the block and remove the cement without breaking it; and whether too thick or too thin, it must remain as it was, it being impossible to alter it. *When I first began to prepare such sections, I had recourse to a process I had practised upwards of fifteen years ago, in preparing thin slices of the most fragile substances, as calcareous spar, in order to examine their effects on polarised light. My process was simple and of easy execution, and consisted of cementing with duly concentrated Canada balsam, the flattened surface to a piece of plate glass, and then grinding down with emery on a plate of copper. The glass and cement being both transparent, I could, with certainty, determine when I had arrived at the proper degree of thinness, and, when that was attained, nothing more was requisite than to polish the surface. At Mr Witham's request, I wrote a full account of all the manipulations requisite in my method of preparing thin fossil sections, which is printed towards the end of that gentleman's work.*

Explanation of Plates.

PLATE II.

Fig. 1. Transverse section of *Pinus strobus*. *a*. Annual layers.
2. Longitudinal section parallel to a radius.
3. Longitudinal section concentric.
4. Transverse section of *Taxus baccata*. *a*. Annual layer.
5. Longitudinal section parallel to a radius.
6. Longitudinal section concentric.

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* This mode of shewing the structure of fossil woods has been long known; and for years I have been in the practice of recommending it to the attention of geologists.—EDIT.

PLATE III.

Fig. 1. Transverse section of the pale part of *Taxodium disticha*.
a. Annual layer.
2. Longitudinal section parallel to a radius.
3. Longitudinal section concentric.
4. Transverse section of dark part of *Taxodium disticha*.
a. Annual layer.
5. Longitudinal section parallel to a radius.
6. Longitudinal section concentric.

PLATE IV.

Fig. 1. Transverse section of *Araucaria*, from Moreton Bay, New Holland.
2. Longitudinal section parallel to a radius, shewing the double rows of discs.
3. Longitudinal section shewing the single rows of discs.
4. Longitudinal section concentric.

Nota.—Fig. 1, Pl. I, is not intended to represent the most general appearance of the transverse section of Pinus strobus, but that of the particular part of the wood where the longitudinal sections were made.

Establishment of Captain R. Wauchope's Signal for ascertaining the Rates of Chronometers at the Royal Observatory, Greenwich.

DALRY HOUSE,
Edinburgh, 6th December 1833.

MY DEAR SIR,

FEELING much indebted to you for the publicity which has been given through your Journal, January number 1830, to the plan for ascertaining the rates of Chronometers by instantaneous signal, I have much pleasure in mentioning to you that the signal is now established at the Royal Observatory at Greenwich.

Having been deeply impressed with the importance of establishing a signal for ascertaining the rates of chronometers at Greenwich Observatory, from its central situation, I went, when in London last June, to visit the East and West India Exportation Docks and Canal, and found that there were at that time upwards of fifty sail of shipping in the East India Docks fitting for sea, and that there were also many hundred sail of shipping fitting for sea in the West India Docks and Canal, all of which could most distinctly see a chronometer signal from Greenwich Observatory. I, at the same time, conversed with many of the captains and officers of those ships, and found that their mode of getting rates for their time-keepers was most unsatisfactory, many of them trusting entirely to their watch-makers to send a rate along with the chronometer. I need not say how unsatisfactory such rates must ever be found, not only from their *jolting* over the stones before arriving at their destination on board, but the very change from the shore to shipboard renders rates thus obtained very little to be depended upon. In proof of this, the head-master of the Naval School at Greenwich informed me that he not unfrequently took charge of time-keepers, and that he never found the watchmaker's rates at all to be depended on; there being often four or five seconds of error, and sometimes said to be gaining when actually losing, and the contrary.

I found, from all the captains and officers of the shipping with whom I conversed, that a chronometer signal established at Greenwich Observatory would be considered a very great boon to outward bound shipping.

Having ascertained these facts, I stated the same by letter to the Lords of the Admiralty, and the following is their Lordships answer:—

SIR, ADMIRALTY, 20*th June* 1833.

Having laid before my Lords Commissioners of the Admiralty your letter of the 17th instant, I am commanded by their Lordships to acquaint you that your plan for ascertaining the rates of chronometers by an instantaneous signal, has been referred to the Astronomer-Royal. I am, &c.

(Signed) JOHN BARROW.

To Captain WAUCHOPE.

A satisfactory trial having been made of the signal at the Observatory, the following is the public notice issued by the Admiralty upon the subject:—

ADMIRALTY, *October* 28. 1833.

The Lords Commissioners of the Admiralty hereby give notice, that a ball will henceforth be dropped every day from the top of a pole on the eastern turret of the Royal Observatory at Greenwich, at the moment of 1 o'clock P. M., mean solar time. By observing the first instant of its downward movement, all vessels in the adjacent reaches of the river, as well as in most of the docks, will thereby have an opportunity of regulating and rating their chronometers.

The ball will be hoisted halfway up the pole at five minutes before 1 o'clock, as a preparatory signal, and close up at two minutes before 1. By command of their Lordships,

JOHN BARROW.

I cannot forbear adverting once more, as I did in a letter which appeared in one of your former Numbers, to the high degree of usefulness which would accrue, not only to the shipping in Leith Roads, but to the public in general, from the Observatory on the Calton Hill adopting the same method of publishing the true mean solar time as that which has now been adopted at the Royal Observatory at Greenwich. I remain, &c.

R. WAUCHOPE,
Captain Royal Navy.

To Professor JAMESON.

MEMOIR ON THE QUESTION,—WHETHER ANY LAND ANIMALS HAVE CEASED TO EXIST SINCE MAN'S FORMATION; AND WHETHER MAN HAS BEEN CONTEMPORANEOUS WITH SPECIES NOW LOST, OR APPEARING NO LONGER TO HAVE REPRESENTATIVES ON THE EARTH? *By M. MARCEL DE SERRES.*

WE have already proved, by a great number of facts, that human bones and various objects of art being found in muddy deposits, where species that are now lost are also discovered, this medley could occur, only because these species had become extinct since the appearance of man. We may, however, arrive at the same conclusion by other proofs than geological facts,—and this we shall now endeavour to demonstrate.

These proofs may be drawn, 1*st*, from the representations of the animals on the monuments of antiquity, which, endowed with all the conditions necessary to the possibility of their existence, have no longer representatives on the earth; whence fol-the natural conclusion that they must be lost, like certain of our fossil species or human varieties; and, *2dly*, from the fragments of animals buried in antique tombs, and which are no longer found amongst existing races.

That the first of these conclusions be accurate, it is especially necessary to prove, that the statuaries and painters of antiquity had just and precise ideas of the different conditions of existence to which organization is submitted, and that these artists in all their productions, even in those that appear the most fanciful, have had the imitation of nature in view, and a desire not to wander from the truth. A study of the paintings and statues of the Greek and Roman schools, not only with the interest which their beauty excites, but still more with the intention of discovering their correctness, soon conveys to the mind of a naturalist the most entire confidence. Indeed, this confidence should not be less than that which we grant to the drawings of our modern artists, destined to produce the new species which we are discovering every day.

The antique monuments of Egypt, and still more those of Greece and Italy, present a great number of sculptured and figured animals. Some of these belong to real species, others to imaginary beings. These latter, however, are not solely the creatures of fancy. The different parts of which they are composed are always portions or halves of real and existing animals, each of which gives an exact representation of the animal which it represents. If, then, in the composition of these imaginary beings, the ancients have so closely followed nature, can we doubt that they have done it for the real beings, all whose parts they have combined, in such a way as not to represent different animals?

If among these last, there are some of which no traces can be found upon the earth, ought we not to conclude that their species are extinct, since the era of history? For apprehending the justness of this conclusion, it will suffice to prove, that the artists of antiquity had exact ideas of the relations which exist betwixt the different parts of the same animal, and the end it was to accomplish. Though they had not constructed a complete system of doctrine, yet they at least constantly applied the principle in the representation of the beings, the remembrance of which they wished to perpetuate. A process so enlightened, and so rigorously deduced from the collection of facts, exhibits a power of observation in the ancients, the extent and accuracy of which we recognise, in proportion as we study and comprehend the works they have left us.

Our first object, then, will be to inquire whether the painters and statuaries of antiquity, have maintained a real correctness in the representation of the different animals they have reproduced on their monuments. We shall reserve till another time the enumeration of animals, whether sculptured or engraved, which, endowed with all the necessary conditions of existence, seem, like certain species buried in the bowels of the earth, to have no longer now any representatives on the globe. Before, however, publishing this work, we have thought it necessary to give a list of the animals, and of some vegetables, which are so well represented on the monuments of antiquity as to be recognised, and to announce, that they must have been designed from nature.

Cuvier, whom we consider as the Aristotle of modern times, seems to have been the first to demonstrate the necessary connection which exists between the reciprocal relation of forms, and the end which the living being had to fulfil; or, in other words, the conditions of existence to which it was subjected. He was the first to proclaim the fruitful principle, that every organized being constitutes a *whole*—a system, one and complete—whose parts mutually correspond, and concur in the same definite action, by a reciprocal action. "None of these parts can change," says he, "unless the others also change; consequently each of them, taken separately, indicates and gives all the others."

Accordingly, if the intestines of an animal are organized only for the digestion of flesh, and recent flesh, its jaws must be formed so as to devour prey; its claws to seize and tear it; its teeth to cut and divide it; the whole system of the organs of movement to pursue and catch it; and that of the organs of sense to observe, and from a distance to recognise it; and, finally, that nature shall have placed in its brain the instinct necessary for hiding itself, and ensnaring its victims.

All these conditions change, if the animal be destined to browse, instead of being carnivorous; and to such an extent, that the least articulation of bone, and the smallest epiphysis, has a determinate character relative to the class, order, genus, and species, to which they belong. So that whenever we possess even the extremity of a bone well preserved, we can, by applying, and following out the rules of analogy, and of minute comparison, determine the place the being ought to occupy in the animal series, almost as certainly as if we possessed the entire individual.

It is true the ancients knew not the necessity of the relation of forms and organization, with the end for which the animal was created; or rather, they had not made it the basis of a theory pregnant with important results. But since in every thing that relates to the exact observation of nature, they had apprehended ideas of it sufficiently just and precise, they never swerved from it in practice.

Nor were statuaries and painters the only individuals who had indulged in this kind of meditation; philosophers and poets entertained ideas equally just regarding it. We have only to read their writings to be convinced that they had discovered the groundwork of the necessity of sensible affinities, to which modern geology owes the greater part of its discoveries, and of its most beautiful results.

We are surprised to find these relations so accurately pointed out by a poet, whose elegance scarcely leads us to expect depth of thought, and very minute observation of nature. Anacreon, in his Ode on Woman, ever proves to us that he knew how to ally things that were most opposed. Thus, he has well remarked, that horses alone have solid feet, that carnivorous animals, and especially lions, are the land animals that can, to the widest extent, open their jaws, so presenting the greatest opening for teeth; these are his own expressions. He had moreover observed that animals with horns had cloven feet, and that the timid *glires* are distinguished by their agility of foot. Thus the poet, in passing the different animals in review, characterizes them by traits so precise, that each is indicated by the characteristic sign of its kind.

These ideas, accurate as they are, were probably put forth by Anacreon as facts, which had only been unveiled to him in a kind of rapture, whilst, in the school of Socrates, to which Plato and Aristotle contributed so much splendour, they had been arranged into a general system. Truth, then, sometimes appears to men of genius as in a dream, and they exhibit it previous to its demonstration.

The principle of the concatenation of forms had, we say, been erected into a general system in the school of Socrates; we may at least suppose so, when we see this philosopher conceive the principle of final causes, and maintain, that, in nature, every thing in its place contributes to the harmony of the whole, and to the formation of the grand chain which ascends from the rudest animals to God himself.

This principle is the same with that of the conditions of existence,—or that of the suitableness of parts, and of their arrangement in harmony with the part the animal has to fulfil in nature; an important principle, as we have before observed, whence results the possibility of certain resemblances, and the impossibility of certain others,—a principle truly reasonable, whence that of analogy of plan and composition is deduced with an accuracy which demonstrates its justice.

No principle so true and so general has yet been discovered applicable to bodies destitute of life. Indeed, it is remarkable, that in all epochs of history, the science of organized bodies has been more advanced than that of unorganized. We conclude, then, that the ancients had some theoretic perceptions, and some just practical ideas on the law of nature, concerning the co-relativeness of forms, of which, in our times, Cuvier has made so many beautiful applications.

That we may the better apprehend it, let us first examine those imaginary beings, which, connecting themselves with the ideas of mythology, may seem at first to recall nothing real, but to be the product of an imagination, as fantastic as it is disordered. When we come to study these strange beings with care, we recognise that they represent in each of the parts that compose them, an exact imitation of nature, and a faithful representation of her laws.

I. *Concerning Mythological Beings represented or sculptured on Antique Monuments.*

The first of these beings with which we shall engage our attention are the centaurs and the tauro-centaurs: After these will follow the minotaur, the satyrs, the fauni, the Pans, the Ægipans, and the *faunisques.* Both of these classes exhibit some portion of the human body, having been represented either with the head or with the trunk of man. The mythological beings to whom the ancients have given the head or body of our race, or both together, have never received extremities in accordance with their superior portions; that is to say, though they may have the head or the trunk of a man, they never exhibit his feet. It is the same with all those, who, like the sphinx, the sirens, harpies, tritons, and the naiades, have any thing human about them. The Greek and Roman statuaries do not appear to have ever deviated from this rule, whilst those of ancient Egypt, much more free, and less constrained by precise rules, have but little regarded it.

It is necessary to make this remark, that we may accurately comprehend the monuments of antiquity. With this exception, the extremities or the limbs of these mythological beings always exhibit themselves in accordance with the supposed habits or manners of these divinities.

The ancients, then, have arranged the different portions of their fabulous beings so as to represent them as requiring the laws of an organization conformable to the habits which they assign them. Accordingly, the forms they have chosen never contradict these laws. In truth, when these beings are intended to represent ruminating animals, their extremities are always such as agree to this class of animals; and it is quite the contrary, when, in their compositions, they have wished to retrace the *solidungula* or the *monodactyles.*

Regarding centaurs, hippo-centaurs, and ono-centaurs, with which we are at first to occupy our attention, it is known that the ancients constructed their monsters half men, half asses or horses. Supposing them thus formed, they have never given them the paws of *bisulcated* or of ruminating animals, but those which suit with the monodactyles or solidungula. They have constantly assigned to them the solid paws which characterize this class of animals.

On the other hand, when they have represented their bu-centaurs or their tauro-centaurs—monsters, half men and half oxen or bulls, they have given them the head of those animals with the trunk of a man. But, in accordance with the rule they had imposed on themselves, the ancients have bestowed on them extremities in keeping with the conformation of their heads, viz. cloven feet, resembling those of the ruminating animals which have horns.

The same principles have directed them in the formation of their minotaurus, which, with the head of man, presents the body of a bull. This head would have required human feet; but as they had imposed on themselves the law never to give such feet to these symbolical beings, not even when they exhibited a human head, it was necessary, along with the body, which represented the minotaurus, to depict the feet of a ruminating animal: this, accordingly, is what the painters of ancient Greece and Rome have invariably done. It is scarcely necessary here to repeat, that as the artists of Egypt have observed no rule in the invention of their mythological beings, no advantage would accrue from our studying them in the point of view that now engages us.

There is, however, an exception to these principles, or, at all events, the appearance of one. It is supplied in the centaur, which, according to the statement of Pausanias, had been represented on the famous coffer of Cypselus, whose anterior extremities resembled the feet of man, and were therein in conformity with the trunk which supported the head of the centaur. Many antiquarians have likewise remarked, that similar centaurs, with atlanter extremities, like human feet, have been represented on some monuments. As, however, they have not pointed out these monuments, we have not hitherto been able to satisfy ourselves whether their assertion be correct. Supposing it true, it perhaps makes an exception to the general rule, since these fabulous beings in a manner exhibit two trunks.

We shall also find the same attention bestowed in harmonising the whole of the frame of the other symbolical beings, the products of the brilliant imaginations of the poets and artists of antiquity. Thus, for example, their satyrs, their fawns, and their other rural divinities, such as the pans, the ægipans, and the *faunisques*, calculated to represent the lubricity of goats, have been pictured by them, as men with shaggy hair, having horns on their brow, and goats' ears; far from giving them solid feet, they have constantly figured them with goats' tails and the cloven feet of *bisulcated* tribes.

The affinities of the relation of forms have been so well understood by the artists of antiquity, that possibly they might have remarked the relations of the dental with the muscular system, as also the constant harmony which exists between organs apparently unconnected. It is at least certain, that they had observed that whenever animals had horns, they had also cloven feet. All the designs which they have left us of real or imaginary beings, with simple or branched horns, present the organs of motion in harmony with this particular, and this is true even when the figure represents horns on human heads.

As it regards real beings, on the other hand, such as lions, tigers, leopards, hyenas, wolves, foxes, and many other carnivorous animals, drawn or sculptured on the monuments of antiquity, the entire whole, as well as the details of the minutest parts, exhibit themselves in keeping with the parts these animals fulfil in nature.

Of this we may convince ourselves, by glancing at the different monuments the ancients have left us to prove the power of harmony. Orpheus, supposed by them to be enchanting the different animals, as much by the charm of his voice, as by the melody of his lyre, is, as it were, surrounded by it. All present there, their distinctive characters. The carnivorous, the *pachydermata*, the *glires*, the *solidungula*, and the *ruminantia*, are there marked by their particular and characteristic traits. They have them with sufficient accuracy, so as to be easily recognised; as we can recognise their divinities by their respective attributes. It even happens that the ancient artists have extended this accuracy to considerations long neglected by naturalists, notwithstanding their importance.

Thus they have remarked, that, in ruminating animals, with horns and branches, the form and arrangement of these parts was far from being the same, and that their differences might contribute to distinguish them. The natural result of this observation, has led them to represent these different parts with the greatest possible accuracy. Sometimes their attention was directed to parts less essential; and thus, for example, they have not forgotten faithfully to depict the tails of certain animals. Thus, when they have represented the *pachydermata* of the boar kind, they have invariably characterized them by a tail bent back, and turned upon itself. In the representations they have left of their hunting exploits, exhibitions which present a great number of different animals, among which we mention lions, tigers, leopards, boars, hippopotamuses, deers, elks, hares, rabbits, bulls, horses, and dogs of different races, whether pointers, mastiffs, or greyhounds, we can especially convince ourselves of the care they took to give to each kind, as to each race, its characteristic peculiarities.

We have already said, that the ancients carried this accuracy not only into the representation of real beings; it also directed them in the composition of fantastic and allegorical beings.

The sirens and harpies, monsters half women and half birds, are another proof of it. The former, occupied without ceasing in slaying the unfortunate individuals who had been attracted by the sweetness of their song, of course required to be organized in such a manner as to satisfy their cruel instinct. The painters and statuaries, then, in representing them with a woman's head, have given them the body and the feet of a bird, and of a bird of prey. Some artists, however, have depicted the sirens with a woman's head and chest, with wings at the shoulders, and the inferior part of the body terminating as does that of the marine mammiferæ. It is thus that, on the medals of Cumæ, the siren Parthenopé is represented. Still, as the sirens thus formed might have been very easily confounded with the naiades, few of the statuaries and painters have adopted this mode of conformation.

The artists of antiquity have likewise represented the harpies with the countenance of a woman and the feet of a bird. The feet terminate in crooked claws, like the vultures, so expressing a ravenous disposition, the distinguishing characteristic of the harpies. Others, in depicting them with the human head, hands, and feet, have always preserved the vulture's wings, adding at the same time the ears of the bear, animals whose gluttonous propensities perhaps cannot be surpassed, except by those which the ancients have attributed to the monsters before us.

Their destructive propensity was also announced by the helmet or buckler, with which they sometimes armed these symbolical beings. But, with such an armour, the ancients were never guilty of the folly of combining the feet of the *grallæ*, or of the *palmipedes*, birds whose mild and timid manners could not accord with those of the sirens and the harpies. On the contrary, in the different groups meant to represent the fable of Jupiter and Leda, the swan, whose form the Master of thunder had borrowed, invariably presents the beak and the feet of a *palmiped.* It could not be thus with the *Stymphalides*, birds mysterious, but necessarily carnivorous. The statuaries, in conformity to the ferocity of disposition they ascribed to them, have represented them with a strong and sharp beak, and feet armed with crooked claws. As a consequence of their careful tendency to exact imitation of nature, they have never given them spurs. It is known that this defence is never found on birds with crooked claws,—a fact which the ancients could not certainly have divined by *a priori* reasoning.

Struck with the affinities which exist between the organization and the destiny of an animal, the ancients have maintained them even in their most fanciful compositions, but without insisting too much upon them, as they have done respecting the facial angle. They had truly remarked with accuracy, that the human head assumes its highest degree of grandeur and beauty, when the facial angle, in harmony with the other parts, approaches the right angle; whilst beyond that, it merges into the absurd, losing its imposing presence, in proportion as it is distant from the 90°.

In applying this rule, and in all its consequences, to the statues which recall the features of the Master of Olympus, or those of Apollo in the beauty of youth, the ancients have not however made it the result of a theory, which perhaps, without the ability of Camper, we shall have yet to conjecture.

But how is it that the modern statuaries, better acquainted than those of antiquity, with the rules by which the human figure can receive all the beauty of which it is susceptible, are yet left so far behind the models and master-pieces struck out by the chisel of the antique masters? It is because, in the imitative arts, once arrived at the beau-ideal, it is impossible to go further; and to reach it, genius is a surer and more powerful guide than the most positive rules. In fact, in the fine arts, as in literature, the model has always taken the lead of rules and their application. It is certain, then, that, in the imitation of nature, the ancients have maintained an accuracy and a vigour which discloses a genius as judicious as it is deep.

After dwelling on these creatures of fancy, so calculated to characterize the genius of the ancients, we shall direct our attention to their sphinxes, their griffons, their tritons, their naiades, and their sea-horses, so often reproduced upon their monuments. We shall still find in them the same dependencies that they have established in their various allegorical existences.

Thus the Sphinxes, the symbol of strength and prudence, had nearly invariably a human head with the body and paws of the lion. The griffons, analogous to the eagles or vultures, were also supposed to be armed with strong and crooked nails, such as we observe on the most decidedly carnivorous mammiferæ (the feline tribes); whilst the sea-horses had always the feet of the *solidungula*, as in their Pegasus, which differs not from the common horse except in his wings, whereby he mounts into the skies.

The tritons and the naiades, sea divinities, known, at least the former, by the shell or the trumpet which they approach to their lips, had bodies terminating like the Cetacea, that is to say like the mammiferæ which inhabit the waters of the ocean. This is also true of the naiades, when the ancients made them creatures, half women and half fishes, understanding this word not in its rigorous accuracy, but as indicating animals residing in the bosom of the waters. Other statuaries or painters of antiquity, have, on the contrary, represented the naiades as women of exquisite beauty; then, they have given them all the elegance their imagination could attain, enveloping them in slight veils, playing in the winds.

Even in the *Chimæra*, that monster composed of many parts of different animals, we find the bias of the genius of the ancients towards that which is true. In truth, if we examine, one by one, the parts of which they construct this monster, we shall find them in accordance with those they ought to recall. Thus, whether they give it the tail of the dragon, and the body of a goat, whether the head and body of the lion, in the middle of which they may raise the head of a ruminating animal, and a tail terminated by that of a serpent, they scarcely ever deviate from the truth in each of the parts of a combination so extraordinary.

The relations that exist between the organization of an animal, and the end it has to fulfil, are far from having been so judiciously appreciated by the painters and statuaries of ancient Egypt. The animals which these artists have wished to represent on their monuments, present neither the purity of form, nor the exact imitation of nature, which we remark on those which the Greeks and Romans have left.

However, in those paintings and sculptures which reach back to the epoch of Egypt's greatest prosperity, they nearly constantly give cloven feet to animals with horns. So when they figured quadrupeds whose jaws were armed with sharp and pointed canine teeth, the Egyptian artists took care at the same time, to furnish them with the feet of carnivorous animals. As, however, we do not always trace in their works, an aiming at the rigorous and precise observations of true forms, even could we find, on their monuments, animals uniting all the conditions of existence, without appearing to have representatives at present on the earth, we could not be so certain that these animals have had a real existence, and belong to the species now regarded as lost. This conclusion appears to us correct, only in relation to animals which, depicted on the monuments of Greece and ancient Rome, are no longer to be met with on the surface of the globe.

In spite of this conclusion, it ought to be remarked, that, in the monuments of ancient Egypt, there exist more than fifty different animals, so exactly designed as to be recognised at the first glance. They belong to nearly all the classes.

The statuaries and painters of modern times have also endeavoured to invent creatures of fancy. But as they have not taken sufficient pains to compose them out of portions really true, their compositions generally present nothing agreeable nor graceful. So thoroughly is it based in reason, that it is truth alone that delights, and, to avail ourselves of the expression of the poet, "nothing is beautiful but the truth."

The Greek and Roman artists have not confined themselves to the representation of the different terrestrial mammiferæ which were known to them. They have given the same attention to the figures which they have left us of reptiles, birds, fishes, the crustacea, and the insects which had attracted their regards. They have given the same attention to vegetables, and especially to trees. The slightest attention suffices to recognise upon these monuments the olive, the oak, the palm, the pomegranate, the laurel, different kinds of pine, the vine, ivy, barley, wheat, the lotus, the melon, many kinds of poppy, amongst which we may mention the wild-poppy, and a crowd of others which it would be too tedious to enumerate.

These artists carried their precision so far, that, for example, the astragalus of different of the ruminating animals—the little bones (tali) which they used in their minor games (ludi minores), have been exhibited by them in such exact fidelity, that we may, by the help of this bone alone, recognise the species to which it had belonged*. So likewise the *belemnites*, which are called thunder-stones, had so attracted their attention, that we find them on their monuments, along with the different animals and vegetables of which we have been treating.

II. *Of real beings, actually existing, painted or sculptured on the monuments of antiquity, whose species can be recognised.*

The assiduity with which the ancients have followed nature even in the composition of their mythological beings, manifests how much care they must have given to the representation of the real beings they had continually before their eyes. So it is that we find it easy to recognise the species when we direct our attention to the figures of the animals which they have left us. This pursuit had already exercised the learning of several antiquaries, particularly of Winckelman and of Millin†, and in availing ourselves of their labours, we have very much extended the list of figured animals which they have given us. These animals belong principally to the terrestrial mammiferæ, which by their size and importance have been more frequently represented than those of other families. Therefore, on the monuments of antiquity also, these terrestrial mammiferæ are in much greater number, than animals of the other classes; and it is easy to apprehend the reason of it.

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* See especially a letter upon bronze medals, published at Rome in 1778, under the title, "Nummis aliquot æreis uncialibus epistola," and forming a volume in 4to.

† Description des pierres gravées du Baron Stosch; par Winkelman,

But we find a great number of living species both of animals and vegetables, so exactly represented on the monuments of the ancients, that it is difficult not to admit that they have been designed from nature, and from the living specimens; for the greater number of them present not only their distinctive characters, but also the gait and the carriage which suit them. This accuracy is so great, that, after the example of many naturalists and antiquarians, we have not been able to prevent ourselves from reposing confidence in it. If we repose this confidence on the figures depicted by able artists in modern times, how can we refuse it to those which have been traced by men of approved abilities, who were less influenced by preconceived ideas? This confidence which we have in the artists of antiquity, has prompted us to furnish in this place a catalogue of the various species they have depicted; and as antiquarians cannot fail to partake of the interest of the inquiry, they will assuredly extend the list farther than our position has enabled us to do.

The ancients have not limited themselves to the representation of the different species merely which they wished to depict; they have given no less attention to those lesser varieties which we have denominated races. We have only to cast our eyes upon their monuments to perceive, that they have accurately distinguished the various races of the domestic animals, particularly those that are observed amongst cattle, sheep, dogs and horses. As to these last, they have marked the differences which exist between the draught and the saddle horse, and they

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Florence 1760. 1 vol. 4to. Dissertation sur quelques médailles des villes grecques, qui offrent la représentation de l'objets d'histoire naturelle; par Millin. Magasin Encyclopédique, t. v.

have indicated these differences as well in their paintings, as in their statues.

The monuments of antiquity are not the only proofs we can adduce to demonstrate that the ancients had very exact ideas of the different varieties of domestic animals, and amongst others of the horse. The war-horse, for example, which Xenophon has described in detail (De Re Equestri, I. 1.) has nothing in common with the herd of horses which are represented on some monuments. On the contrary, it is represented on the Parthenon; in equestrian statues; in some Greek bas-reliefs, also on Trajan's pillar, and sculptors have received this as the model of the war-horse. It is this model Virgil had in view in the Georgics (III. 72, 88), and Varro in his immortal work, *De Re rustica.*

The horses represented on the medals of Carthage do not belong to the same race, and they also differ much from those which we see on the medals of Alexander Thaos, and Achelaus, king of Macedon. Those which we observe on the coins of Syracuse, and the medals of Philistis and of Gelo have but a very remote resemblance to the preceding races. The horses designed upon the monuments of Persepolis exhibit the race of Persia, very different from that of Egypt, which are figured on the ancient monuments of Thebes. This last variety has the greatest alliance to the war-horses described by Xenophon, and which came from Thessaly, as also with the bronze horses of Venice, and those that are upon the friezes of the Parthenon. Though we may not hope to meet on the ancient monuments with all the remarkable races of horses which Oppien has described, and which amount to fifteen, it will, nevertheless, be possible to discover the greatest number of them.

These facts which we have pointed out, sufficiently prove the attention which the ancient statuaries have bestowed on the various races of domestic animals. We have, therefore, judged that we might dispense with other similar researches, notwithstanding the great interest which they afford.

Finally, besides the artists of the Greek and Roman schools, and some Egyptian, there are few who have represented the different animals with any fidelity. Thus, for example, the hieroglyphic paintings of the Americans, and particularly those of the Aztiques, whom Humboldt has made us acquainted with in his Observations on the Aborigines of America, do not exhibit a faithful imitation of nature. In truth, on many of these monuments tigers and leopards are figured with the feet of the *Solidungula*, a combination and error which the least scrupulous artists of ancient Rome, and antique Greece, would never have committed.

(To be continued.)

Description of several New or Rare Plants which have lately flowered in the neighbourhood of Edinburgh, and chiefly in the Royal Botanic Garden. By Dr Graham, Professor of Botany in the University of Edinburgh.

10th Dec. 1833.

Ceropegia Lushii.

C. *Lushii;* radice tuberosa; caule volubili; foliis lanceolato-linearibus, subprismaticis, breviter petiolatis; pedunculis axillaribus, multifloris, foliis multo brevioribus; corollæ tubo medio cylindraceo, limbo erecto, piloso; coronæ segmentis interioribus erectis, filiformibus, fundo corollæ inclusis, exterioribus patulis, truncatis.

Description.—*Root* a flat tuber, producing from its crown several long twining, fleshy, glabrous, filiform stems, of reddish-grey colour. *Leaves* (1½-4 inches long, one-fourth of an inch broad near the base) opposite, on short petioles, lanceolato-linear, gradually tapering to a long narrow point, glabrous, subprism-shaped, slightly undulate on the edges, channelled above, subglaucous. *Peduncles* axillary, cymose, greatly shorter than the leaves, many-flowered. *Calyx* 5-parted, segments acute. *Corolla* (three-fourths of an inch long) leaden coloured and glabrous on the outside; tube globular at the base, cylindrical above, deep purple and slightly hairy within; limb 5-parted, segments shorter than the tube, erect, connivent and cohering at the apex, reflected in the edges, so as to expose the inner surface, which is of deep purple, and covered with spreading hairs of the same colour. *Crown* white, wholly included within the globular base of the corolla, inner segments erect, filiform, alternating with the outer segments, which are much shorter, spreading, truncated, concave upwards. *Anthers* blunt; pollen-masses erect, yellow. *Pistil* equal in length to the calyx, blunt, greenish-white, broadly grooved on the sides between the angles.

The tuber of this plant I received from my friend Dr Lush of Bombay in February last. It flowered in the stove of the Royal Botanic Garden in September.

Eriostemon gracile.

E. *gracile;* frutex humilis, ramulis pendulis; foliis clavato-semicylindraceis, cumque petalo unguiculato parce tuberculatis, sparsis, numerosis; floribus terminalibus, solitariis; filamentis monadelphis, versus apicem lanatis.

Description.—A low shrub, with pendulous, twiggy, very leafy, subverticillate branches, bark brown and scarred. *Leaves* (2 lines long) fleshy clavato-semicylindrical, attenuated at the base, scattered, ciliated, somewhat tubercled, spreading wide, leaving elevated scars when they fall off. *Flowers* (when expanded 10 lines across) terminal, solitary. *Calyx* 5-parted, segments short and blunt, subglabrous. *Petals* 5 (4 lines long) spreading wide and reflexed in their upper half, elliptical, channelled in front, unguiculate, lilac, estivation imbricated, their thickened backs glandular, but without pubescence, whilst the sides are slightly pubescent, but with few or no glands. *Stamens* 10, of unequal length alternately, all shorter than the petals; filaments glabrous and monadelphous for more than the lower half of their length, above this they are densely woolly within and without, wool somewhat yellow, at the upper part of the smooth portion there are on the outside a few pellucid glands; anthers all perfect, cream-coloured, oblong, connective projecting a little above the cells. *Pistil* shorter than the stamens; stigma green, with 5 erect blunt lobes; style stout, reddish-brown, hairy, glabrous towards the stigma; germen ovate, 5-lobed, dark green, longer than the calyx, and nearly as long as the style, with a few hairs at the apex of each lobe. *Ovules* ovate, two in each lobe.

This rather graceful little shrub was raised from seed imported from New Holland by Mr Cunningham at Comely Bank Nursery, Edinburgh, and has flowered freely during the last two years in a border within the greenhouse. The whole has a resinous perfume, approaching some of the *Diosmas* too nearly to be agreeable.

Francoa sonchifolia.

F. *sonchifolia;* caulescens, foliis lyratis, decurrentibus, lobis distantibus alâ sinuatâ sursum angustatâ conjunctis; racemo spicato, erecto; pedunculo pedicellisque pubescentibus.

Francoa sonchifolia, *Ad. Juss.* Ann. des Sciences Nat. 3. 192. t. 12.—*Spreng.* Syst. Veget. 2. 262.—*D. Don* in Ed. New Philosoph. Journ. 1828.

Llaupanke amplissimo sonchi folio, *Feuill.* Journ. 1. 742. t. 31.

Panke sonchifolia, *Willd.* Spec. Pl. 2. 487.

Description.—*Stem* erect (2½ feet high) suffruticose, succulent and slightly pubescent above, round. *Leaves* lyrate, undulate, pubescent on both sides, bright green, semiamplexicaule, decurrent for a little way, lobes blunt, undulate, dentate. *Peduncles* axillary and terminal, greatly elongated, round, pubescent, branched, the branches rising from the axil of a diminished leaf. *Raceme* spicate, erect, very long and handsome. *Pedicels* rising from the axils of lanceolate, entire *bracteæ*, and rather shorter than them, pubescent, spreading when in fruit. *Flowers* suberect. *Calyx* 4–5-cleft, as long as the peduncle, pubescent, persistent. *Corolla* of 4–5 petals, spreading, more than twice as long as the calyx; petals spathulato-oblong, lilac, darker in the centre. *Stamens* 8–10, equal in length to the calyx, alternating with an equal number of much shorter sterile filaments. *Stigma* 4–5-lobed, sessile, peltate, spreading, attached to the apex of a central column, lobes blunt. *Germen* oblong, 4–5-sided, 4–5-celled, deeply furrowed between the lobes, which project upwards in acute angles around the stigma, ovules very numerous, receptacle central. *Capsule* elongated, erect, septicidal. *Seeds* oblong, testa remarkably wrinkled.

This species is at once distinguished from *Francoa appendiculata* by its stem—the flowers are very similar. In describing *Francoa appendiculata* (New Phil. Journ. June 1832, and Botanical Magazine, fol. 3178.), I made, though doubtfully, the same reference as here given to Ann. des Sciences Nat.; but I now believe the absence of stem in the single specimen described by Jussieu to have arisen from its having flowered when young. No. 826. of Cumming's Herbarium, which seems to me, on several accounts, a very distinct species, has the leaves of *F. appendi-*

ciliata, yet a distinct stem. *F. sonchifolia* is a large branching plant, and, in the greenhouse of Mr Neill, produced flowers in succession during the greater part of July and August. It was first raised by Mr Menzies, Halifax, Yorkshire, from seeds sent from Chili.

Frasera carolinensis.

Gen. Char.—*Cal.* quadripartitus, patens. *Corolla* calyce multo major, quadripartita, patens, laciniis elliptico-ovatis, glandulâ medio barbatis. *Stamina* corollâ breviora, antheris inferne bifidis. *Stigmata* duo crassa, glandulosa. *Germen* ovatum, tetragonum, compressum, in stylum brevem attenuatum, uniloculare; ovula 8–12. *Capsula* ovalis, valde compressa, subcartilaginea, unilocularis, margine bivalvis. *Semina* 8–12, elliptica, membranaceo-marginata.

Frasera Carolinensis, *Walt.* Flor. Car. 88.—*Gmel.* Syst. Nat. 2. 256.—*Pers.* Synops. 1. 137.

Frasera Walteri, *Mich.* Fl. Bor. Americ. 1. 97.—*Pursh*, Fl. Americ. Sept. 1. 101.—*Roem. et Schult.* Syst. Veget. 3. 162.—*Nutt.* Genera, 1. 103.—*Elliot*, Bot. of S. Carolina and Georgia, 1. 205.—*Spreng.* Syst. Veget. 1. 428.—*Torrey*, Fl. of Mid. and North. States, 1. 187.

Description.—*Stem* (5 feet high) erect, panicled at the top, stout, internodes cylindrical, of rich purple colour, glabrous. *Leaves* (the lowest above 1 foot long, more than 3 inches broad) glabrous, entire, verticelled, 5 in a whorl, elliptico-ligulate, smaller and more ovato-acute upwards, and, towards the top, 4 in a whorl. *Peduncles* (or *branches*) axillary, resembling the stem, having opposite leaves of the same appearance as those on the upper whorls of the stem, in the axils of each of which 3 single-flowered green and glabrous pedicels arise, of which that on each side next the leading shoot of the branch first elongates and expands its flower, the others in succession from it outwards, the terminal flower of each branch, however, being expanded before any of the others. *Calyx* 4-parted, green, glabrous, segments linear-subulate, spreading wide, reflected at the points. *Corolla* (1½ inch across) 4-parted, nearly flat, greenish-yellow, spotted with purple within, segments ovato-elliptical, each having a large, circular, green and hairy gland towards its base, at which place on the outside they are generally purplish before expansion. *Stamens* 4, shorter than the corolla, and adhering to it at the base, erect, diverging above; filaments stout, and somewhat concave at the base, closely embracing the lower part of the germen, glabrous; anthers versatile, green, oblong, cleft at the lower extremity nearly to the insertion of the filament, at their upper bi-mucronate, opening along their edges; pollen yellow, granules extremely minute, nearly spherical. *Stigma* bilabiate, segments spreading, short, thick, greenish-yellow. *Style* single, glabrous. *Germen* twice the length of the style, glabrous, glaucous-green, 4-sided, subcompressed, two of the angles being more acute than the others, unilocular. *Ovules* about 12, parietal, attached alternately to the edges of the two valves on both sides of the loculament.

We have had the specimen here described for four or five years in an open border at the Botanic Garden, having received it from Mr Smith of Ayr. It flowered in June and July, and it yet remains doubtful whether it will perfect seed. If it do not, we shall lose the species, as its life will terminate with the period of flowering. It is desirable in cultivation, and its shape is handsome, but the colour is rich only on the stem.

Hypericum hyssopifolium.

H. *hyssopifolium;* herbaceum; floribus pyramidato-paniculatis, trigynis; calyce obtuso, cumque petalis contortis subserratis glanduloso-ciliato, glabro, pellucido-striato; foliis glaucis, decussatis, elliptico-linearibus, sessilibus, cumque petalis pellucido-punctatis, integerrimis, glabris, subnervosis; caule ramoso, ascendente, tereti, costis duabus inconspicuis.

Hypericum hyssopifolium, *Willd.* Sp. Pl. iii. 1470.—*Pers.* Synops. ii. 91.—Bot. Mag. 3277.

Hypericum hyssopifolium, γ, foliis latioribus planiusculis.—*Bieberst.* Fl. Taurico-Caucas, 2. 231.

Hypericum alpestre, *Fischer.* in litt. fide Hooker.

Description.—*Stems* (1½ foot high) many from the crown of the root, ascending, much branched, pyramidal, panicled above, glabrous, round, marked with two opposite obscure ridges alternating in the internodes. *Leaves* (9 lines long, 3 lines broad) decussating, sessile, elliptico-linear, glaucous, glabrous, spreading wide, channelled in the middle, and having a few faint lateral veins, entire and slightly revolute in the edge, dotted with minute pellucid points. *Panicle* large, terminal; peduncles 3-flowered, the lateral flowers opposite, and arising from the axils of diminished leaves. *Calyx* segments ovate, blunt, marked with pellucid streaks, fringed with black glands. *Corolla* (an inch across when expanded) much contorted; petals spreading, clawed, obovate, unequal, nerved, pellucido-punctate, subserrate, ciliated with black glands; claws longer than the calyx. *Stamens* erect; filaments much shorter than the corolla, yellow, irregularly connate at the base; anthers incumbent, greenish-yellow. *Stigmas* small, of many minute red points. *Styles* three, diverging, yellow. *Germen* ovate, 3-lobed, reddish, striated, afterwards of deeper red, 3-locular. *Ovules* oblong, very numerous, attached to the central receptacle.

This very pretty species was raised at the Botanic Garden from seed communicated by Dr Fischer of St Petersburgh without specific name. It flowered freely in the open ground in June 1833.

It appears from Bieberstein that it is a native of the mountains of Tauria. He conceives the leaves to be very variable in breadth, the extremes of which are illustrated in his var. β and γ. There is no difference among our seedlings.

Lobelia odorata.

L. *odorata;* caule herbaceo, procumbente, radicante, ramoso, glabro; foliis ellipticis, sparsim serrato-dentatis, planis, glaberrimis, petiolatis; floribus axillaribus, solitariis, erectis, pedunculo foliis breviore, postea elongato; calycibus superioribus, glabris, segmentis integerrimis.

Description.—*Stem* procumbent, slightly channelled, glabrous, branched, rooting at every leaf, and forming a dense tuft. *Leaves* (4–6 lines long, 2–3 lines broad) distichous, elliptical, with few coarse serratures, glabrous, flat, veined, on short petioles. *Flowers* axillary, solitary, on petioles which are erect, at first shorter than the leaves, but longer when passing into fruit. *Calyx* superior, erect, glabrous, segments entire. *Corolla* perfumed like hawthorn, erect, white; tube cleft to its base, thrice as long as the calyx, covered with spreading hairs within; limb 5-parted, segments equal, lanceolate, spreading. *Filaments* inserted into the base of the corolla, white, purplish at the top, hairy especially in their lower half, hairs reflexed. Tube of anthers leaden coloured, bi-aristate on their lower side. *Pistil* longer than the stamens; stigma bilabiate, purple, fringed at its base, lobes reflected, blunt, mucronate; style glabrous above the anthers, everywhere else covered with spreading hairs; germen top-shaped, inferior, glabrous or rarely finely pubescent, terminated with a yellow glabrous disk. *Ovules* very numerous, placentæ large. Unripe seeds ovate, slightly compressed, pale brown, dotted.

Seeds of this plant were sent to Mr Neill, Canonmills, in 1832, from Mr Tweedie, and flowered in the stove at Canonmills during the autumn. It possesses but little beauty beyond that of a lively green turf. Its perfume is remarkable in the genus.

Lupinus incanus.

L. *incanus;* caule suffruticoso, ramoso; foliis digitatis, foliolis lineare lanceolatis, utrinque sericeis, integerrimis, subcarinatis, petiolo duplo brevioribus; racemo elongato, pedicellis æqualiter sparsis, patulis; calycibus basi nudis, labio superiore bi-, inferiore tridentato; vexillo alis breviore, emarginato; leguminibus immaturis erectis, sericeo lanatis, apicibus approximatis.

Description.—Whole *plant* silky, excepting only the corolla, stamens and style. *Stem* suffruticose, erect, branched. *Leaves* (about 6 inches across) digitate, leafets about 9, linear-lanceolate, silky on both sides, carinate below, entire, very acute. *Petiole* nearly twice the length of the leafets, compressed vertically. *Stipulæ* (1¼ inch long) adhering for about half their length, subulate. *Raceme* (1½ foot long) terminal, elongated. *Pedicels* scattered equally over the rachis to within a little way of its base, which is naked, spreading when in flower, when in fruit erect, springing from the axil of a subulate, caducous bractea. *Calyx* bilabiate, the upper lip 2- the lower 3-toothed, bibracteate towards the base, bractea small, subulate, adpressed, inconspicuous. *Corolla* pale lilac; vexillum reflected upwards, and at the sides, subrotund, emarginate, cordate at the base, orange, and slightly spotted in the middle, keeled behind, claw short and rigid; alæ rather longer than the vexillum, straight in the upper edge, curved in the lower, and there cohering towards the apex, slightly turned up at the point, claws short; keel half the length of the alæ, more rigid and shining than the other parts of the flower, and of deeper purple colour at its point, which is raised above the upper edge of the alæ, dipetalous, petals cohering only near their apices. *Stamens* monadelphous, included, the five with rounded anthers only a little longer than the others; anthers orange-coloured, pollen granules very small and nearly spherical. *Pistil* longer than the stamens; stigma very small, capitate; style subulate, glabrous, shining; germen silky; ovules several. Unripe *Legume* erect, silky-woolly, subcylindrical, tapering and connivent at their apices.

This species was raised by Mr Neill from seed sent by Mr Tweedie of Buenos Ayres, and flowered freely in the greenhouse at Canonmills in June 1833. It approaches very near to *Lupinus multiflorus* of Encyclop. Methodique, vol. iii. p. 624, and, had it not been for the very conspicuous pedicels in Mr Neill's plant, I should scarcely have separated them.

Nuttallia Papaver.

N. *Papaver;* foliis radicalibus lobatis palmatisve, caulinis inferioribus palmatis, superioribus simplicibus digitatisve; calycibus pilosis, involucratis; involucro triphyllo, foliolis lanceolatis pilosis.

Nuttallia Papaver, Bot. Mag. 3287.

Malva Papaver, *Cavan.* Diss. ii. p. 64. t. 15. f. 3.—*Pers.* Synops. 2. 251. *De Cand.* Prodr. i. 431.—*Spreng,* Syst. Veget. 3. 92.

Description.—*Stems* numerous from the crown of the root, ascending, slender, slightly hairy, hairs adpressed. *Root-leaves* on very long petioles, lobed or pedate, thinly sprinkled on both sides and on the edges with harsh hairs. Lower *stem-leaves* palmato-pedate, upper digitate or simple, nerved, all slightly hairy, lobes linear-lanceolate; lobes of the root and lower stem-leaves more or less inciso-pinnatifid. *Stipulæ* ovate, acute, ciliated. *Peduncles* very long, axillary, single-flowered. *Involucrum* 3-leaved, closely surrounding the calyx, leafets lanceolate, hairy. *Calyx* 5-cleft, segments ovate, acute, 3-ribbed, hairy; hairs spreading, acute, rising singly from tubercles placed on the ribs or edges of the segments. *Corolla* of five petals, campanulate, large, red-purple; petals obovato-cuneate, truncated, and unequally crenato-dentate at their extremity, edges at their base woolly. *Stamens* very numerous; filaments united into a somewhat hairy conical tube for about two-thirds of their length; anthers kidney-shaped, reddish-yellow, single-celled, opening along the vertex. *Pistil* at first shorter than the stamens; stigmas linear, reddish, hairy, decurrent along the inside of the numerous deeply divided at length protruded segments of the style; germen depressed, glabrous, cells arranged in a circle, yellowish, each emarginate on the outside, and within extended into a dark-green blunt apex. *Ovules* solitary in each cell, reniform, attached by the sinuosity, and pendulous.

I hesitated about describing this plant as a distinct species, fearful that in the genus there may be a strong disposition to vary. So many plants, however, have flowered in different gardens around Edinburgh, some from imported roots, others from seed, as in Mr Cunningham's nursery, Comely Bank, at Mr Neill's, and at Mr Falconar's Carlowrie, and all with precisely the same characters, that I believe it will be thought at least as distinct from either of the two already in cultivation as they are from each other. The figure published in the Botanical Magazine was drawn from a very vigorous plant in Mr Neill's greenhouse, which has been in flower ever since the beginning of August, and has still, at this late period of the season (17th December) several flowers upon it. The presence of the involucrum brings the genus too near *Malva*, into which I strongly suspect it should fall. My desire for this reduction is increased by the discovery of the synonyme of Cavanilles, for which I am indebted to Dr Hooker, as well as for pointing out that Persoon, De Candolle, and Sprengel, have copied a blunder of Willdenow's, who marks Lusitania instead of Louisiana, the station affixed to the plant by Cavanilles. We are, therefore, as Dr Hooker very justly remarks, indebted to Mr Drummond, not only for the addition of a very pretty plant to our gardens, but for the means of clearing up a doubtful species. Cavanilles described and figured his plant as having only two leafets in the involucrum; but this of course arose from his description having been made from one, and, as it would seem, an imperfect specimen.

Viola pedata, var.

V. *pedata;* stigmate incrassato hinc obliquè truncato, rostro brevissimo, foliis pellucido-punctatis palmatisectis, stipulis pectinatim laceris longe adhærentibus, petalis omnibus glabris.—*De Cand.*

Viola pedata, *Linn.* Sp. Pl. edit. 2. p. 1323.—*Willd.* Sp. Pl. i. 1160.—Bot. Mag. t. 89.—*Michaux*, Fl. Bor. Americ. ii. 151.—*Andrew*, Bot. Repos. t. 153.—*Pers.* Synops. i. 254.—*Poiret*, Encycl. Meth. viii. 625. *Pursh*, Fl. Americ. Sept. i. 171.—*Nutt.* Genera, i. 147.—*Roem. et Schult.* Syst. Veget. v. 351.—*Elliott*, Bot. of S. Carolina and Georgia, i. 300.—*De Cand.* Prodr. i. 291.—*Bigelow*, Flor. Boston. 96.—*Torey*, Fl. of the Middle and Northern Sections of the United States, i. 249. —*Spreng.* Syst. Veget. i. 797.

Viola Virginiana tricolor, foliis multifidis, cauliculo aphyllo. *Pluk.* Alm. 388. t. 114. fig. 7.

Viola digitata, *Pursh*, Fl. Amer. Sept. i. 171.—*Roem. et Schult.* v. 351.

Viola ranunculifolia? *Poiret*, Enc. Meth. viii. 626.—*Roem. et Schult.* v. 352.

DESCRIPTION.—*Root* having a large fleshy crown. *Leaves* (1½ inch long, 2 inches across) all radical, flat, somewhat fleshy, covered on the upper surface with minute pubescence, glabrous and shining below, having many minute transparent dots, strongly nerved, nerves prominent on both sides, but especially the upper, the outer leaves often cuneate, truncated, crenate in the centre, inciso-crenate at the sides of the abrupt termination, the others pedate, 5–7-lobed, lobes spathulate, 3–5-toothed at the apex, or entire. *Petioles* (2–3 inches long) rounded below, flat above, and there having a strong prominent rib. *Stipulæ* subulate, pectinato-ciliate, adhering to the dilated base of the petiole for nearly half their length. *Scape* (5 inches high) erect, longer than the leaves, glabrous, channelled on its inner side, bracteate a little way above the base; *bracteæ* opposite, subulate, toothed, gibbous at the base. *Calyx* glabrous, green; leafets shortly auricled (the two lowest every way the largest) broad and truncated at the base, tapering to a point at the apex. *Corolla* large and very handsome; lowest petal pale lilac, white in front near the claw, obcordate, pendulous, shortly spurred; the other petals erect or reflected, elliptical, somewhat oblique, the two centre ones, and nearly the upper half of the side ones, dark velvet-purple, behind all the petals are pale lilac, nearly uniform. *Stamens* free, and anthers nearly without pollen in the specimen described, the two lowest longer than the others, apices oblong, blunt, concave on the inside, orange-yellow. *Pistil* equal in length to the lowest stamens, everywhere glabrous; germen conical, green; style clavate; stigma obliquely truncated, very shortly rostrate.

This plant is extremely different in its appearance from the specimen figured in Botanical Magazine, t. 89, but I do not find any characters by which it can be considered specifically distinct. It is probably the variety mentioned by Pursh as having a handsome corolla, variegated with pale blue and dark purple velvet; but this is quoted by De Candolle, on the authority of Rafinesque, as his *β*, which has a pubescent pistil, whereas in this it is perfectly glabrous. I have quoted *Viola ranunculifolia* with a slight degree of doubt, merely because it is described as glabrous, which this is not on the surface of the leaves. The plant is extremely beautiful, and highly deserving of cultivation. It was introduced by Mr Drummond from Georgia into the Botanic Garden, Glasgow, from whence we received it in 1832. With us, it produced in the greenhouse a succession of flowers in the beginning of October 1833, and it is now (10th December) about to flower again.

PROCEEDINGS OF THE ROYAL SOCIETY OF EDINBURGH.

(Continued from Vol. IV. p. 402.)

March 18.—Professor Russell, Vice-President, in the Chair. The following communications were laid before the Society:—

1. Observations on the Anatomy of the Rorqual (a Whalebone Whale of the largest magnitude), drawn up from the dissection of a specimen found dead off North Berwick. By Robert Knox, M. D., F. R. S. Ed.

This paper, composed chiefly of anatomical details regarding the anatomical structure of the Rorqual, scarcely admits of abridgment. The author has described the skeleton of the cavity for receiving the brain and the mechanism of the larynx at greatest length. The entire length of the whale, measured by a straight line, drawn on the sand from the nose to the middle part of the tail, and making a slight allowance for the curved position in which the animal lay, was 80 feet. Length of the head 23 feet. The girth of the carcass at the pectoral extremities (though the animal had been ten days on the beach, and was much collapsed) 34 feet. Breadth of the tail from tip to tip 20 feet. The author describes the appearance of the mouth, lined with whalebone, as very surprising. The whole surface of the palatal plates of the superior maxillary bones, each extending to 14 feet in length, was covered with a mass of what appeared to be well teased baked hair, of a clear and shining black. This was the whalebone arranged in the most regular manner, and composed of many thousand plates; the number as seen in profile, and which are the largest plates, amounted to upwards of 650. It weighed nearly two tons while soft. The whole skeleton weighed nearly thirty-two tons, and was removed to Edinburgh with much difficulty. The weight of the brain, calculated by Sir William Hamilton's method, from the capacity of the cranium, must have been about fifty-four lb.

The larynx is quite simple, and totally unlike that of the Dolphin and Porpoise. The nostrils are filled by two enormous cartilaginous masses acted on by muscles occupying the centre of the superior maxillary bones. When the animal breathes, they are withdrawn sideways to admit the passage of air. This extraordinary structure the author considers as unique, and that it had not fallen previously under the notice of any scientific observer.

2. Dr Knox verbally communicated some new observations on the structure of the Foot of the Horse. He demonstrated the navicular bone of the horse's foot not to be a sesamoid bone, nor a peculiar structure formed expressly for the horse, but the *Epiphysis* of the *Os pedis* or coffin-bone. This was proved satisfactorily by a direct appeal to structure. Besides anticipating results of practical consequences from this discovery, the author is led to observe, that an organ may be displaced and employed to perform different functions in different animals,—that the epiphyses of bones are intended by nature to form separate bones in a vast variety of animals,—and that they may often lead to the discovery of the type of the skeleton in fossil remains of extinct animals.

3. The reading of a paper was commenced, entitled, Experimental Researches regarding certain Vibrations which take place between Metallic Masses having different Temperatures. By James D. Forbes, Esq. Professor of Natural Philosophy in the University of Edinburgh.

1833, *April* 1.—Sir Thomas Makdougall Brisbane, President, in the Chair. At this Meeting the following communications were read:—

1. Continuation of "Experimental Researches regarding certain vibrations which take place between Metallic Masses having different Temperatures." By James D. Forbes, Esq. Professor of Natural Philosophy in the University of Edinburgh.

The vibrations here referred to, are those which with their accompanying sounds were first observed by Mr Arthur Trevelyan, and communicated to this Society in a paper published in the 12th vol. of their Transactions. The author of the present paper undertook the inquiry as soon as the remarkable fact was announced by Mr Trevelyan, and was induced to prosecute it to a considerable extent experimentally, in consequence of being dissatisfied with the only plausible explanation yet offered,—that of the successive expansions of the cold metal by the hot one at the point of contact at each successive vibration, which was conceived to afford the necessary impulse or maintaining power.

In this paper, the phenomena of sound are first discussed, which, with Mr Faraday, the author imputes solely to the number of vibrations taking place in a given space of time. This seems completely proved by observation, and the note depends upon the frequency of the oscillations, which have been observed as high as between 700 and 800 in a second, and must often be greatly more frequent. The phenomena of vibration are next considered as affected by the nature of the metals, by the form of the masses, and by temperature. The order of the metals as vibrators, is the following, meaning that the cold metal must always stand lower in the list than the hot one, and that the force or intensity of vibration is, generally speaking, proportional to the space intervening between two metals on the list, *Silver, Copper, Gold, Zinc, Brass, Platinum, Iron, Tin, Lead, Antimony, Bismuth.* Antimony and Bismuth are placed at the bottom of the list, because no other metal is capable, under any circumstances which have been examined, of producing vibrations in conjunction with those two metals: they are the only metals yet observed which, when heated, do not vibrate on cold lead.

From experiments detailed at considerable length in the paper, the author is led to the following practical conclusions, which, whatever may be the fate of the hypothesis which he is disposed to found upon them, will, he conceives, be viewed as valuable in themselves

1. As far as has been observed, the vibrations never take place between substances of the same nature. 2. Both substances must be metallic. 3. The vibrations take place with an intensity proportional, within certain limits, to the difference of the conducting powers of the metals for *heat* (or *electricity*,*) the metal having the least conducting power being necessarily the *coldest.* 4. The time of contact of two points of the metals (between which the oscillations take place) must be longer than that of the intermediate portions. 5. The impulse is received by a distinct and separate process at each contact of the bar with the block, and in no case is the connection of these points in any way essential. 6. The intensity of vibration is (under certain exceptions), proportional to the difference of temperature of the two metals.

From these data, the author first endeavours to show, that the hypothesis of expansion is untenable, by tracing closely the process of communication of heat, and proving that it must lead to several conclusions totally at variance with experiment, and particularly that as far as conducting power for heat is concerned, both the hot and the cold metal should possess it in the highest degree. The author is led by the striking analogy of the powerful repulsive action of electricity in passing from a good to a bad conductor, to infer a similar property in heat, which, without entering into any speculations as to the nature of those principles, appear to have a repulsive character in common indicated by a tendency to diffusion and equilibrium. He conceives, that while some very delicate experiments in France have given indications of the actual force exerted by heat equally diffused through two adjoining masses, that the energy in this case is produced by the accumulated repulsive power in the last particles of the good conductor, the current (without meaning any thing hypothetical by the term) being suddenly cut short by the resistance opposed by the inferior conductor to its passage. The destructive energy of electricity indicative of its repulsive force, is never exerted in a state of equilibrium, but by the accumulation of separate repulsive energies which take place in the transition from a good to a bad conductor, or during its passage through the latter.

2. On the Equations of Loci traced upon the surface of the Sphere, as expressed by spherical co-ordinates. By T. S. Davies, Esq. F. R. S. E.

This paper is intended as a necessary supplement to the paper bearing the same title already printed in the Society's Transactions, though but an abridgment of a larger one which the author had prepared on the subject. Particular circumstances induced him to alter the plan he had originally contemplated, and instead of a complete development in detail of his researches and his views, he has only on the present occasion given so much of his results as were necessary to bring the system of polar spherical co-ordinates to a state analogous to that in which plane polar curves has long been stationary, one point of the analogy excepted, viz. where the author has extended the method of treating tangents and normals, and the consequent investigations dependent on these, by giving the polar equations of those lines, instead of merely examining the relation between the radius-vector, and perpendicular upon the tangent. In a note the equations of the tangent and normal, to plane curves, is given from first principles; and the analogy between plane and spherical curves is shewn to be remarkably close.

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* See the abstract of a former paper on the identity of those arrangements.

Amongst the properties of spherical curves, the following curious one occurs.

If the pole of a loxodrome to rhumb α be made the centre of another equal sphere, the visual cone under which the loxodrome will appear, will cut the second sphere (the eye being at centre of first sphere) in a curve, whose equatorial subtangent is constant, and equal to α: and if conversely, the pole of the spherical logarithmic (the equisubtangential curve just mentioned) be made the centre of an equal sphere, the visual cone of this logarithmic, seen from the centre of its own sphere, will cut the second sphere in a loxodrome whose rhumb is equal to the subtangent of the logarithmic.

The author expresses his intention of discussing in a separate work the singular points of spherical curves, certain new systems of co-ordinates, and other classes of research, which, on account of the length to which they necessarily extend, were not adapted to the Transactions of the Royal Society.

3. Experiments and Observations on the Arterialization of the Blood. By William Gregory, M.D., F.R.S.E. and W. J. Irvine, Esq.

The object of these experiments was to ascertain some of the circumstances under which the blood changes in colour, from the dark venous to the florid arterial hue.

Dr Stevens first showed, that the venous clot deprived of its serum by washing, remained dark even when in contact with oxygen; and that the addition of a saline solution caused it immediately to become florid. He stated also, that a strong saline solution would cause this change in an atmosphere so highly charged with carbonic acid as to prove rapidly fatal to animals.

It appeared to the authors necessary to ascertain whether this effect took place in pure carbonic acid, containing no free oxygen, as the atmosphere used by Dr Stevens most probably did; and they also proposed to extend their observations to other gases, likewise free from oxygen. They accordingly prepared pure nitrogen, hydrogen, and carbonic acid, removing the last traces of oxygen by means of potassium. The water with which the clot was washed, and the saline solution used in the experiments, were deprived of atmospheric air by being boiled and allowed to cool in close vessels. The clot was then introduced into the gases over mercury, and as soon as the strong saline solution came in contact with it, the colour, in all the three gases, changed from black to bright red, and the same change was found to take place in the Torricellian Vacuum. It was obvious, therefore, that a strong saline solution could change the colour of the blood from venous to arterial without the contact of oxygen, or indeed of any gas whatever.

But in blood, the colouring matter is in contact, not with a strong saline solution, but with a very dilute one, viz. the serum. It was necessary, therefore, to see whether the washed clot, placed in contact with serum or a weak solution of salt, in the same gases, would change its colour. On repeating the experiments, both with serum and a solution of salt in water of equal strength to the serum, no change whatever took place, until atmospheric air or oxygen gas was admitted.

The conclusions of Dr Stevens, therefore, must be somewhat modified. It is true, as he states, that the presence of saline matter is essential to the change of colour: But it is obvious, that there is an essential difference between that change as it occurs in the lungs, where serum is present, and as it appears out of the body when a strong saline solution is employed. In the former case, oxygen is necessary: In the latter, the change of colour is independent of the presence of any gas whatever. We must, therefore, be cautious how we reason by analogy from the one class of phenomena to the other.

April 15.—Professor Russell, Vice-President, in the Chair. The following communications were read:—

1. Observations on the Lines of the Solar Spectrum, and on those produced by the Earth's Atmosphere, and by the Action of Nitrous Acid Gas. By Sir David Brewster, LL. D., F. R. S.

The author was led, in prosecution of his researches on the absorptive action of transparent media of light, which have been partly communicated in previous papers to the Society, to examine the influence of coloured gaseous bodies. Iodine vapour was one of these, and its action was found of a similar character to that of fluids having a similar tint. Nitrous acid gas presented a far more extraordinary phenomenon.

The Spectrum of Newton, and of all the philosophers of the 18th century, was a parallelogram of light with circular ends, in which the *seven* colours gradually shaded into each other without any interruption. The illumination was a maximum in the yellow rays, and the light decayed by insensible degrees towards the red and violet extremities. In the year 1808, Dr Wollaston conceived the happy idea of examining a beam of day-light that passed through an aperture only the 20th of an inch wide, and he was surprised to see it crossed by seven dark lines, perpendicular to its length.

About ten or twelve years afterwards, the celebrated optician, Joseph Fraunhofer, without knowing what had been done by Dr Wollaston, observed the spectrum formed by the sun's light transmitted through small apertures; and by applying a telescope behind the prism, he discovered about 600 parallel dark lines traversing the spectrum. As no such lines appeared in the spectra of white flames, Fraunhofer considered them as having their origin in the nature of the light of the sun. The strongest of these lines were seen in the spectra of the Moon, Mars, and Venus; and by means of very fine instruments, he was able to detect one or two of them with other new lines in the spectra of Sirius and Castor.

Upon examining with a fine prism of rock-salt, with the largest possible refracting angle (nearly 78°), the light of a lamp transmitted through a small thickness of nitrous acid gas, whose colour was a pale straw-yellow, the author was surprised to observe the spectrum crossed with hundreds of lines or bands far more distinct than those of the solar spectrum. The lines were sharpest and darkest in the violet and blue spaces, fainter in the green, and extremely faint in the yellow and red spaces. Upon increasing, however, the thickness of the gas, the lines grew more and more distinct in the yellow and red spaces, and became broader in the blue and violet, a general absorption advancing from the violet extremity, while a specific absorption was advancing on each side of the fixed lines in the spectrum. It was not easy to obtain a sufficient thickness of gas to develope the lines at the red extremity, but the author found that heat produced the same absorptive power as increase of thickness; and by bringing a tube containing a thickness of half an inch of gas to a high temperature, he was able to render every line and band in the red rays distinctly visible.

The power of heat alone to render a gas which is almost colourless as red as blood without decomposing it, is in itself a most singular result; and the author succeeded in rendering the same pale nitrous acid gas so absolutely black by heat, that not a ray of the brightest summer's sun was capable of penetrating it. In making the experiment, the tubes frequently exploded; but by using a mask of mica and thick gloves, and placing the tubes in cylinders of tinned iron with narrow slits to admit the light, there is little danger of any serious accident.

The author then points out various practical applications which may be made of this discovery, especially its substitution for the more difficult process, when Fraunhofer's lines in the solar spectrum are employed, of determining the dispersive powers of substances. Since the absorptive action by increasing the thickness of the medium generally *enlarges* the lines already defined, these may be rendered as distinct as may be required, which it is impracticable to do with the solar lines, and hence the difficulty of applying these to useful purposes. The lines in the sun's light, and those of the nitrous acid gas spectrum, when directly compared, have a strong analogy; but in order to establish it completely, the author found Fraunhofer's map of the solar spectrum insufficient, and was induced to undertake the laborious task of going over the whole ground. By dint of perseverance, and by the use of some original methods, he has been enabled, with very inferior instruments, to distinguish about 2000 lines instead of the 354 which Fraunhofer had laid down.

The author watched narrowly the state of the defective solar lines at different seasons of the year, in order to observe if any change took place in the combustion by which the sun's light is generated, or in the solar atmosphere through which it must pass. Such changes he found to be very general in every species of terrestrial flame. The definite yellow rays which exist in almost all white lights, flicker with a variable lustre, and analogous rays in the green and blue spaces proceeding from the bottom of the flame, exhibit the same inconstancy of illumination. In the course of the winter observations, he observed distinct lines and bands in the *red* and *green* spaces, which at other times wholly disappeared; but a diligent comparison of these observations soon shewed that these lines and bands depended on the proximity of the sun to the horizon, and were produced by the absorptive action of the earth's atmosphere.

The atmospheric lines, as they may be called, or those lines and bands which are absorbed by the elements of an atmosphere, have their distinctness a maximum when the sun sinks beneath the horizon. The study of them consequently becomes exceedingly difficult in a climate where the sun, even in a serene day, almost always sets in clouds; but the author has been able to execute a tolerably accurate delineation of the atmospheric spectrum.

Most of the lines thus widened by the atmosphere, are faint lines previously existing in the spectrum.

The author's observations, whilst they indicate the remarkable fact, that the same absorptive elements which exist in nitrous acid gas exist also in the atmospheres of the sun and of the earth, lead us to anticipate very interesting results from the examination of the spectra of the planets. Fraunhofer had observed in the spectra of Venus and Mars some of the principal lines of the solar spectrum. This, indeed, is a necessary consequence of their being illuminated by the sun; for no change which the light of that luminary can undergo is capable of replacing the rays which it has lost. But while we must find in the spectra of the planets and their satellites all the defective lines in the solar spectrum, we may confidently look for others arising from the double transit of the sun's light through the atmospheres which surround them.

2. Notice relative to the Pigmentum Nigrum of the Eye. By Thomas Wharton Jones, Esq.

The objects proposed by the author of this paper are,—

1. To correct certain opinions prevalent with regard to the membrane of Jacob, this having been frequently confounded with another, the structure of which forms the immediate subject of the paper.

2. To shew that the *Pigmentum Nigrum* is not a mere mucus or varnish exhaled by the surfaces on which it is found, but is deposited in a membrane distinct from the choroid, which possesses a peculiar structure hitherto unknown. This membrane, being the seat of the pigment, but not the pigment itself, which may or may not be present, the author proposes to call the *Membrane of the Pigment.*

If a portion of this membrane be examined by the aid of the microscope, it is seen to consist of very minute hexagonal plates, in which are deposited numerous black particles, which are to be considered as properly constituting the pigment, but not essential to the hexagonal plates composing the membrane, because these may, and do, exist without the black particles.

In the eye of the Albino Rabbit, the author found, as he had *a priori* expected, the membrane of the pigment to exist. The plates composing which, however, are still less developed than those of that part of the membrane which lies over the tapetum, in the eyes of the horse, ox, &c. They are in fact not hexagonal, but circular, a structure similar to which the author has found in the eye of a very young human fœtus.

Behind and around the ciliary processes, and on the posterior surface of the iris, the membrane of the pigment ceases to present the hexagonal structure, although still composed of small irregularly rounded masses of about the same size as the hexagonal plates, to which they are evidently analogous.

This change in the structure of the membrane of the pigment, which is only partial in the eyes of the Mammiferæ, the author has found to obtain in its whole extent, in the eyes of those animals lower in the zoological scale which he has examined, except in the eye of the Cuttlefish, in which there is an approach to the hexagonal structure in that part of the pigment which lies on the posterior surface of the part in which the crystalline lens is fixed.

May 6.—Sir Thomas Makdougall Brisbane, President, in the Chair. The following communication was read:—

On the Composition of some Iron Slags. By J. F. W. Johnston, Esq. F. R. S. E.

This paper described the composition of some crystallized slags from Birtley Iron-Works, in the County of Durham. These slags, like those from Wales described by Professor Miller in the Cambridge Transactions, had the form of the olivine, and in composition were nearly pure silicate of iron, containing about 0.4 per cent. of foreign matter, chiefly magnesia. The author also gave a short account of a method by which the magnet might be made available in giving immediately very near approximations to the quantity of iron contained in the basic silicates of iron.

At the Birtley Iron-Works, the author stated that beautiful crystals of titanium had likewise been found.

The Society then adjourned to the general meeting in November.

Dec. 16.—Sir Thomas Makdougal Brisbane, Bart. President, in the Chair.—

Communication relative to the Fresh-Water Limestone of Burdiehouse, near Edinburgh, belonging to the Carboniferous group of Rocks. By Dr Hibbert.

In his paper, the author states that the limestone in question, which is confounded with the common carboniferous or mountain limestone of marine origin, is, in his opinion, of fresh-water origin.

On an irregular line extending from Joppa on the coast of the Firth of Forth, in a south and south-west direction to the Pentland Hills, strata of mountain or carboniferous limestone crop out at intervals; and their marine origin is indicated by encrinites, the Productus, &c., and corallines. This limestone is developed with the least interruption between Edmonstone and Muirhouse, where it is from twelve to twenty feet thick or more. Along this part of the line may be seen fractures and elevations of the strata of limestone and superincumbent shale and sandstone, evidently occasioned by a sudden and violent uplifting force acting from north-east to south-west, and causing the strata which were the subject of this convulsion to dip south-east at an angle of 25°. These uplifted beds, between Edmonstone and Muirhouse, and subsequently to Burdiehouse, form the strata which dip under the coal-measures of Gilmerton, Loanhead, and other sites.

At Muirhead Quarry the same mountain limestone is seen, but a covered state of the ground succeeds in the same south-west direction for a mile, in which no outcropping strata are observable, except some beds of sandstone about the middle of that space, dipping with the other strata towards the south-east at an angle of 25°, and placed higher than the limestone.

At the south-west termination of this space is situated the Quarry of Burdiehouse. It is difficult to determine, from the covered state of the ground, whether the bed of limestone here seen is lower or higher than the mountain limestone hitherto described; possibly the mountain limestone may here thin off, and be replaced by the Burdiehouse bed. The appearance of the strata, however, which crop out between Burdiehouse and Loanhead, dipping near the former site towards the south-east at an angle of 30°, and towards the latter at a less angle in the same direction, shew that the limestone of Burdiehouse, in common with the mountain limestone cropping out between Edmonstone and Muirhead, is lower, and therefore of older formation than the coal-measures of Gilmerton and Loanhead. Hence the limestone of Burdiehouse and the mountain limestone of marine origin, are jointly referable to one common epoch of formation.

The Burdiehouse limestone, however, is clearly not of marine, but of fresh-water origin. It forms a bed of twenty-seven feet in thickness, composed of strata about four and a-half feet thick, dipping south-east at angles of 23° and 25°, with the seams of stratification regular and continuous, and also with intersecting vertical seams.

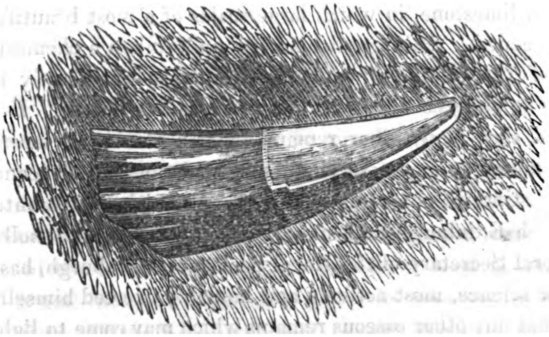
The bed is surmounted by bituminous shale, with which very thin layers of limestone occasionally alternate. The colour of the limestone is grey, brown, and sometimes purple; its fracture conchoidal, but often slaty, from the intervention of thin striæ of vegetable or bituminous matter; its texture hard and compact; its aspect dull, and it is not crystalline like the limestone of neighbouring quarries. It is tolerably pure, shewing little foreign matter except what is bituminous; and this is often disposed between the layers of the limestone where its structure its slaty. But its most remarkable character is the nature of the organic remains contained in it. There are, in the first place, plants belonging to the oldest vegetation of the globe; among which the *Sphenopteris affinis, Sphenopteris bifida, Lepidostrobus variabilis* of Mr Lindley, and various kinds of the Lepidodendron seem to be ascertained. The species are numerous and distinct, so as to afford the most beautiful specimens that can be conceived, and a rich store of observation in fossil botany. But, secondly, the animal remains are even more interesting. One fragment of a fish, which, when entire, must have measured more than a foot in length, seems closely allied to the fresh-water genera of the family of *Cyprinidæ*. Innumerable minute animals referable to the fresh-water *Entomostraca* are also to be seen; one of these is probably a *Cypris*, and indications have been found of minute *Conchifera*, and of coprolites (indicative of large animals) in great abundance.

The inference appears to the author irresistible, that the Burdiehouse limestone is of fresh-water origin. The neighbouring mountain limestone abounds in corallines, encrinites, and shells, all evidently marine. These are in vain sought for in the limestone of Burdiehouse; which, on the other hand, presents the remains of Fish apparently inhabiting fresh-water, and of Ferns, Lycopodiaceous plants, and such aquatic vegetables as flourish most among fresh-water lakes and marshes. This limestone, then, is the memorial of some inland fresh-water lake or tank, within the waters of which it was elaborated.

Additional Particulars relative to the Saurian Remains found in the Burdiehouse Limestone. Communicated in a Letter to Professor JAMESON *by Dr* HIBBERT.

DEAR SIR,—In compliance with your wish, I shall furnish you with a general notice of such other discoveries as have taken place in the Quarry of Burdiehouse since my researches commenced.

On the morning subsequent to my communication being read to the Royal Society of Edinburgh, I revisited the quarry of Burdiehouse, in company with Mr Witham, and, in the course of this visit, one of the workmen accidentally found, inclosed in a fragment of the limestone, a tooth, two inches and a quarter in length, of a large reptile, the form and structure of which were so well exposed in the fracture which the stone containing it had undergone, that I shall not attempt, upon any account whatever, to dislodge it from its matrix. This relic is also in the most beautiful state of preservation that can well be imagined, possessing an enamel of a nut-brown colour, which shines with all the brilliancy of perfect freshness. It is intended, in an engraving, to imitate the lustre and colour displayed by this interesting relic. In the mean time, its shape and magnitude are shewn in the following wood-cut, which I have directed to be made, and which you are at liberty to use for your Journal.



This tooth is evidently of an animal of the Saurian order. I have compared it in its general form and size with one that belonged to a very large skeleton of the fresh-water Gavial of the Ganges, in the museum of Dr Knox, which it was found much to resemble. At the same time it must be observed, that the deep striæ to be noticed at the lower part of the tooth, are more like the marks which characterize the Ichthyosaurus, an animal whose remains have been hitherto found in formations rather indicative of estuaries. But as the limestone of Burdiehouse gives unequivocal evidence of a fresh-water origin, even the presence of Ichthyosaurian remains within it, if such could be shewn, would prove nothing more than the very common incident of such a fresh-water lake or tank communicating with the sea. Any further remarks on this head I shall suspend until more remains of the animal turn up.

During the course of the last meetings held of the Wernerian and Royal Societies of Edinburgh, you have informed me that a tooth like that which I have described, along with other osseous relics, had been noticed by Mr Ure in his History of Rutherglen, who it seems published his work in the year 1793; and on each occasion of your remark I added, that, from a perusal of the Statistical Accounts of Scotland, I had no doubt whatever that in the coal-fields of many other places besides Rutherglen, Saurian remains had from time to time been disclosed. I have compared Mr Ure's delineation of the tooth, which is like the one found at Burdiehouse, but apparently much smaller. It is said to have been discovered in the till *above coal.*

Along with the fossil tooth, I procured at the same time some slight vertebral remains, most probably referable to the same Saurian reptile; and I have now the pleasure to add, that Mr Arthur Connel has, two or three days ago, obtained from the workmen some fragments of limestone, in which large scales of a most beautiful lustre are imbedded. This gentleman has undertaken a chemical examination of the coprolites which are found so abundantly in the quarry. The analysis could not be in more able hands. But it is to be hoped that many other remains, which this most interesting ossiferous deposit promises to yield, may make us better acquainted with the distinctive character of the animal which first haunted the site on which Edinburgh now stands. Mr Robison of Atholl Crescent, General Secretary of the Royal Society of Edinburgh, has, from his zeal for science, most actively and laudably exerted himself, with the view that any other osseous remains which may come to light may be carefully preserved. . . . . . .

This discovery I consider to be a most important one. It refers the existence of reptiles more or less allied to the Crocodile, to a period much earlier than has been usually supposed by geologists, and shews that these immense animals must have existed coeval perhaps with the very earliest vegetable state of our globe. Believe me, dear Sir, yours, &c. &c.

S. HIBBERT.

MANOR PLACE, EDINBURGH,
18th *December* 1833.

PROCEEDINGS OF THE WERNERIAN NATURAL HISTORY SOCIETY.

1833, *April* 6. Rev. Dr DAVID RITCHIE in the chair.—Mr James Wilson read a paper on the natural history of the glow-worm, a colony of which, found in the neighbourhood of Edinburgh, he had made the subject of particular observation. He pointed out the change of habits, in regard to food, which takes place among these insects, at a certain period of their transformations, the larvæ being predaceous, or attacking living prey, particularly minute testacea, and other mollusca, while the perfect insect are herbivorous.

A communication by Mr Macgillivray was then read, regarding the occurrence of a large flock of foreign water-fowl, the *Anas Ægyptiaca*, on the eastern coast of Scotland; but the author suggested the possibility of these birds having strayed from Lord Wemyss's pleasure-grounds at Gossford; and that the present instance could not therefore, with certainty, be regarded as illustrating the natural migration of the species. A drawing was exhibited of the leader of the flock, which had been shot by Captain Sharpe.

An extensive and valuable series of highly finished representations of the indigenous animals of Great Britain, chiefly quadrupeds and birds, by Mr Macgillivray, was also exhibited to the meeting. Professor Jameson pointed out that their peculiar excellence consisted in their combining, with great beauty of pictorial effect, a more accurate representation of the forms of the crania, as always identical in the young and old of the same species,—an important particular, greatly neglected by ornithological draughtsmen; and also in there being less of what is called *mannerism* in the general treatment of the plumage, the characteristic form and texture of the feathers of each species being particularly attended to by Mr Macgillivray.

—— *April* 20. R. JAMESON, Esq. P. in the chair.—A communication from Dr Scouler of Glasgow was read, giving an account of the discovery during last autumn (1832), of two specimens of the *Sorex remifer* of Geoffroy, in the vicinity of that city. They differ from the water-shrew in being of a larger size; of a deep velvet black on the back and sides, and a ferruginous brown beneath, with the tail rounded at its origin, but compressed towards the extremity; the toes ciliated. The most obvious character seems to be the very flat nose or snout, which resembles that of the *Chrysochloris capensis*. The *Sorex remifer* was detected many years ago in Norfolkshire by Dr Hooker, and was figured by the late Mr Sowerby in his Miscellany, under the name of *Sorex ciliatus*. It is singular that the animal is not mentioned in Dr Fleming's work on British Animals.

1833, *Dec.* 14. Dr Charles Anderson, V. P. in the chair. Professor Jameson read Mr Nicol's observations on the structure of recent and of fossil coniferous trees, which were illustrated by an extensive suite of specimens. [This important essay is printed in the present number of this Journal, p. 137, *et seq.*]

Dr Hibbert then gave an account of the discovery of the tooth of a Saurian animal in the limestone of Burdiehouse, three miles south from Edinburgh. Professor Jameson remarked that this tooth differed from those of the Gavial, the Ichthyosaurus, and Plesiosaurus, and that data were still wanting for the exact determination of its true nature, whether belonging to a fish or a saurian animal, its softness and other characters shewing that it could not be referred to the mammalia. [A further notice of this interesting specimen will be found *supra*, p. 193.]

Professor Jameson next exhibited a fossil tooth found in red sandstone, above the coal formation in Berwickshire, by the Right Honourable Lord Greenock, and assigned reasons for regarding it as the tooth of a fish. [See a full account of this curious specimen, by Dr R. E. Grant, *supra*, p. 38.]

At this metting, the following gentlemen were elected office-bearers of the Society for 1834:

President,—Robert Jameson, Esq.

Vice-Presidents,

Dr Gillies.
Dr C. Anderson.
W. C. Trevelyan, Esq.
Dr R. K. Greville.

Secretary,—Pat. Neill, Esq.
Treasurer,—A. G. Ellis, Esq.
Librarian,—James Wilson, Esq.
Painter,—P. Syme, Esq.
Mr W. H. Townsend, *Assistant*.

Council.

Sir P. Walker.
W. A. Cadell, Esq.
Principal Baird.
H. Witham, Esq.
Bindon Blood, Esq.
Dr T. S. Traill.
T. J. Torrie, Esq.
Dr John Coldstream.

PROCEEDINGS OF THE SOCIETY OF ARTS FOR SCOTLAND.

Donation of L. 400 *by Sir David Brewster and Dr James Keith, Trustees of the* KEITH BEQUEST *for the prosecution of Science.*

At a Meeting of COUNCIL, held on 17th April 1833, the Secretary read a letter from SIR DAVID BREWSTER and Dr JAMES KEITH, the surviving Trustees of the fund bequeathed by the late ALEXANDER KEITH of Dunottar and Ravelstone, Esq., dated the 8th of April current, offering to present to the SOCIETY OF ARTS at Whitsunday 1833, the sum of FOUR HUNDRED POUNDS, out of the fund under their management,—to be invested in the manner, for the purposes, and under the conditions mentioned in that letter, which is of the following tenor:—

"To SIR THOMAS DICK LAUDER, Bart.
and the other Members of COUNCIL
of the SOCIETY OF ARTS FOR SCOTLAND.

April 8. 1833.

"GENTLEMEN,—Having been appointed by the late ALEXANDER KEITH, Esq. of Dunottar, Trustees for managing a sum of L. 1000, which he bequeathed for the purpose of promoting the interests of Science and the Arts in Scotland,—we hereby offer to the SOCIETY OF ARTS, at Whitsunday next, the sum of FOUR HUNDRED POUNDS Sterling, to be held in trust by their *Vice-Presidents, Secretary, and Treasurer,* and their successors in office, for the purpose of encouraging the useful Arts in Scotland.

"The principal sum of L. 400, must be invested in the names of the Office-bearers above mentioned, in some public or private security, which shall be approved of by the COUNCIL of the Society.

"This sum is on no account to be encroached upon; but the Interest arising from it is to be applied in sums of money, or medals, in rewarding Inventions, Improvements, or Discoveries, in the Useful Arts, which shall be primarily submitted to the Society.

"We leave it to the discretion of the COUNCIL to determine whether the rewards are to be given in medals, or money, or both; and also to fix upon the periods of their adjudication. But it is distinctly to be understood, that these prizes are to be kept separate from the other prizes adjudged by the Society; that they are to be distinguished by the name of the KEITH PRIZES, and that when they are given in the form of medals, the medal shall bear some inscription, or device, indicating the original DONOR.

"If the Society of Arts should, at any future period, be dissolved, the principal sum of L. 400 shall be re-delivered to the present Trustees in the event of their being both alive; but if only one, or neither of them, shall then be alive, the said principal sum of L. 400 shall be made over to the Principal, and Professors of Natural Philosophy, Chemistry, and Mathematics, in the University of Edinburgh, to be re-invested by them on good security, and the interest to be employed in any manner which they shall think most advantageous for the promotion of the Useful Arts in Scotland. We are, Gentlemen, your most obedient and faithful Servants,

(Signed) DAVID BREWSTER.
J. KEITH."

The Council, on the motion of Mr Allan, seconded by the Rev. Edward Craig, unanimously voted their best thanks to Mr Keith's Trustees for this handsome offer, which the Council agreed to accept on the part of the Society; and at same time directed a suitable answer to be forwarded to the Trustees by the Secretary.

At a Meeting of the Council, held on 15th May 1833, the Secretary read a copy of the answer sent by him, in name of the Council, to the late Mr Keith's Trustees; of which answer the following is a copy:

"To Sir David Brewster, and Dr James Keith, the surviving Trustees of the Fund left by the late Alexander Keith, of Dunottar, Esq., for the promotion of Science.

Edinburgh, 26th *April* 1833.

"Gentlemen,—I have been directed by the Members of Council of the Society of Arts for Scotland, to acknowledge the receipt of your letter of the 8th April last, in which you, as the Trustees appointed by the late Alexander Keith, Esq. of Dunottar, for managing a sum of L. 1000, which he bequeathed for the purpose of promoting the interest of Science and the Arts in Scotland,—make offer to the Society of Arts, at Whitsunday next, of the sum of four hundred pounds Sterling, to be held in trust by their *Vice-Presidents, Secretary* and *Treasurer*, and their successors in office, for the purpose of encouraging the Useful Arts in Scotland; upon the conditions mentioned in your letter.

"The Council of the Society of Arts, upon the part of the Society, accept,—with sentiments of gratitude towards the Trustees of Mr Keith, and of the most respectful regards for the memory of that Gentleman,—the offer which has been made to them;—and at the same time, beg to assure the Trustees, that they shall endeavour, to the utmost of their power, to fulfil and carry into effect the wishes and intentions of Mr Keith and his Trustees, in the Donation which has been made to them. I have the honour to be, Gentlemen, your most obedient Servant,

(Signed) JAMES TOD, *Sec.*"

The Society held its first Meeting for Session 1833–4, in the Royal Institution, on Wednesday the 13th November 1833, at 8 o'clock p. m.

The following communications were laid before the Society during the month of November:—

Nov. 13.—1. Models of two *Machines or Presses for compressing Peat-Earth into Peats.* By Sir Neil Menzies, Bart. Communicated by the Right Hon. Sir John Sinclair, Bart.

2. Drawing and Description of an *Instrument for Cutting Coats,*—constructed on mathematical principles. By Mr William Smith, tailor, Cupar Fife. Communicated by John Govan, Esq. W. S. Edinburgh.

3. Description of a *new and expeditious system of laying on the Colours, and producing light and shade in Drawing and Painting.* By Mr Frederick H. Berkely, Teacher of Drawing (from London), 130, Union Street, Aberdeen.

4. Description and Drawing of an *Improved Steam-Engine for Lifting Water.* By Mr Andrew Symington, Kettle, by Cupar Fife.

5. Donation.—A Brief Narrative, proving the right of the late William Symington, Civil Engineer, to be considered the *Inventor of Steam Land-Carriage Locomotion;* and also the *Inventor and introducer of Steam Navigation.* Edited by Robert Bowie, London, published 1833. Presented by Mr Andrew Symington, Kettle, by Cupar Fife.

Nov. 27.—1. Notice regarding the *Process of Extracting the Whey from the Curd in making Cheese;* and description of a *New Machine for this purpose.* Communicated by John Robison, Esq. Sec. R. S. E., and Memb. Soc. Arts. A Model was exhibited.

2. Drawing and Description of a Machine for *Fishing, Shooting on Water, or Land Travelling*—which can be propelled by Horse, Steam, or Manual Labour, &c.—with a *Back Speed Coupling,* securing the full power of the Lever at all times in going up hill. By Mr William Kent, Dunfermline.

3. Drawing and Description of a *Whitening Engine* or *Gas Reel,* for whitening Yarn with safety. By Mr William Kent, Dunfermline.

4. Description and Drawing of an *Improved Engine Boiler.* By Mr Andrew Symington, Kettle, Fife.

5. Donation.—Specimens of *Lithographic Printing.* By Mr Samuel Leith, Lithographer, Banff, Assoc. Soc. Arts.

The following gentlemen were admitted Ordinary Members, viz.:—

David George Sandeman, Esq. of Springland, 4, Melville Street, Edinburgh.

Robert Allan of Lauriston, Esq. banker, Edinburgh.

STATUTES OF THE UNIVERSITY OF EDINBURGH, RELATIVE TO THE DEGREE OF M. D.—1833.

Sect. I. No one shall be admitted to the Examinations for the Degree of Doctor of Medicine who has not been engaged in medical study for four years, during at least six months of each, either in the University of Edinburgh, or in some other University where the Degree of M. D. is given; unless, in addition to three Anni Medici in an University, he has attended during at least six winter months, the Medical or Surgical Practice of a General Hospital, which accommodates at least eighty patients, and during the same period a course of Practical Anatomy; in which case three years of University study will be admitted.

Sect. II. No one shall be admitted to the examinations for the degree of Doctor, who has not given sufficient evidence,—

1. That he has studied, once at least, each of the following departments of Medical Science, under Professors of Medicine in this or in some other University, as already defined, viz.—

Anatomy,	During Courses of Six Months.
Chemistry,	
Materia Medica and Pharmacy,	
Institutes of Medicine,	
Practice of Medicine,	
Surgery,	
Midwifery, and the Diseases peculiar to Women and Children,	
General Pathology,	
Practical Anatomy, (unless it has been attended in the year of extra-academical Study allowed by Sect. 1.),	

CLINICAL MEDICINE, that is, the treatment of Patients in a Public Hospital, under a Professor of Medicine, by whom Lectures on the Cases are given, ...	During a Course of Six Months, or two Courses of Three Months.
CLINICAL SURGERY, ...	During Courses of at least Three Months.
MEDICAL JURISPRUDENCE, ...	
BOTANY, ...	
NATURAL HISTORY *, including ZOOLOGY,	

2. That in each year of his Academical Studies in Medicine, he has attended at least two of the Six Months' Courses of Lectures above specified, or one of these and two of the Three Months' Courses.

3. That, besides the Course of Clinical Medicine already prescribed, he has attended, for at least six months of another year, the Medical or Surgical Practice of a General Hospital, either at Edinburgh or elsewhere, which accommodates not fewer than eighty patients.

4. That he has attended for at least six months, by Apprenticeship or otherwise, the Art of Compounding and Dispensing Drugs at the Laboratory of an Hospital, Dispensary, Member of a Surgical College or Faculty, Licentiate of the London or Dublin Society of Apothecaries, or a professional Chemist and Druggist.

5. That he has attended for at least six months, by Apprenticeship or otherwise, the Out-Practice of an Hospital, or the Practice of a Dispensary, or that of a Physician, Surgeon, or Member of the London or Dublin Society of Apothecaries.

SECT. III. No one shall obtain the degree of Doctor who has not studied, in the manner already prescribed, for at least one year previous to his Graduation, in the University of Edinburgh.

SECT. IV. Every Candidate for the degree in Medicine, must deliver, before the 24th of March, of the year in which he proposes to graduate, to the Dean of the Faculty of Medicine,—

First, A Declaration, in his own handwriting, that he is twenty-one years of age, or will be so before the day of Graduation; and that he will not be then under articles of apprenticeship to any Surgeon or other master.

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* A Five Months' Course of *Natural History* is given in winter, and one of three months in summer, either of which qualify for a degree in Medicine.—EDIT.

Secondly, A statement of his studies, as well in Literature and Philosophy as in Medicine, accompanied with proper Certificates.

Thirdly, A Medical Dissertation composed by himself, in Latin or English; to be perused by a Professor, and subject to his approval.

SECT. V. Before a Candidate be examined in Medicine, the Medical Faculty shall ascertain, by examination, that he possesses a competent knowledge of the Latin language.

SECT. VI. If the Faculty be satisfied on this point, they shall proceed to examine him, either *viva voce,* or in writing; *first,* on Anatomy, Chemistry, Botany, Institutes of Medicine, and *Natural History* bearing chiefly on Zoology; and, *secondly,* on Materia Medica, Pathology, Practice of Medicine, Surgery, Midwifery, and Medical Jurisprudence.

SECT. VII. Students who profess themselves ready to submit to an Examination on the first division of these subjects, at the end of the third year of their studies, shall be admitted to it at that time.

SECT. VIII. If any one, at these private examinations, be found unqualified for the Degree, he must study for another year two of the subjects prescribed in Section II., under Professors of Medicine, in this or in some other University, as above defined, before he can be admitted to another examination.

SECT. IX. Should he be approved of, he will be allowed, but not required, to print his Thesis; and, if printed, forty copies of it must be delivered before the 25th day of July to the Dean of the Medical Faculty.

SECT. X. If the Candidate have satisfied the Medical Faculty, the Dean shall lay the proceedings before the Senatus Academicus, by whose authority the Candidate shall be summoned, on the 31st of July, to defend his Thesis; and, finally, if the Senate think fit, he shall be admitted, on the first lawful day of August, to the Degree of Doctor.

SECT. XI. The Senatus Academicus, on the day here appointed, shall assemble at Ten o'clock A. M. for the purpose of conferring the Degree; and no Candidate, unless a sufficient reason be assigned, shall absent himself, on pain of being refused his Degree for that year.

SECT. XII. Candidates for Graduation shall be required to produce evidence of their having conformed to those Regulations

which were in force at the time they commenced their Medical Studies in a University *.

JAMES SYME,
Professor of Clinical Surgery,
Dean of the Faculty of Medicine.
W. HAMILTON,
Secretary to the Senatus Academicus.

*** Candidates who commenced their University studies before 1825 will be exempted from the fourth year of attendance (Sect. I.), from the additional Hospital attendance (Sect. II. art. 3.), from the necessity of a year's study in Edinburgh (Sect. III.) and from any attendance on**

Clinical Surgery,	Practical Anatomy,
Medical Jurisprudence,	Pathology, and
Natural History,	Surgery distinct from Anatomy.
Military Surgery,	

Those who commenced between 1825 and 1831 will be exempted from attendance on General Pathology, and also on Surgery distinct from Anatomy.

Those who commenced between 1825 and 1833 will be required to attend only two of the following classes, viz.

Clinical Surgery,	Military Surgery,
Medical Jurisprudence,	Practical Anatomy.
Natural History †,	

And those who commenced before 1833 will be exempted from the attendance specified in Sect. II. Art. 4. and 5.

***N. B.*—The attendance on Midwifery in an University (Sect. II. Art. 1.) is required of all Candidates.**

† The Army Medical Board requires, besides the usual Medical classes, an attendance on—

Natural History, . . .	Three Months.
Botany, ...	Three Months.

LIST OF PATENTS GRANTED AT EDINBURGH FROM 24TH JUNE TO 27TH AUGUST 1833.

1833.

June 24. To Thomas Howard, of Copthall Court, in the city of London, merchant, for an invention of "improvements on his invention denominated the Vapour Engine, and the application of a part or parts thereof, with certain additions or improvements on steam engines."

July 17. To John Hornby Maw, of Aldermanbury, in the city of London, surgical instrument-maker, for the invention of "an improved apparatus for injecting enemata."

30. To William Henson, of the city of Worcester, lace manufacturer, for an invention of "certain improvements in machinery for manufacturing bobbin-net lace."

Aug. 2. To John Scott Russell, M.A., of No. 8. Stafford Street, Edinburgh, for an invention of "certain improvements in the construction of vessels for su[illegible]aining the pressure of fluids, and in the boilers and machinery of steam engines, and in the manner of their application to locomotive purposes."

27. To Thomas Wrigley, of Bridge Hall Mills, near Bury, in the county of Lancaster, paper-maker, for an invention of "an improved pulp strainer, to be used in making paper."

To William Henry James, of Birmingham, civil engineer, for an invention of "certain improvements in the construction of steam carriages, and the apparatus for propelling the same, a part of which improvements is applicable to other purposes."

To George Beale Brown, of New Broad Street, in the city of London, merchant, for an invention in consequence of a communication made to him by a foreigner residing abroad, of "certain improvements in machinery for making or manufacturing pins of the kind which are commonly used for fastening wearing apparel."

Mr Arnott's communication on Indian Plants, we regret, did not reach us in time. Colonel Silvertop's Spain in next Number; also various notices of Books, &c.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

ON THE THERMOMETRICAL STATE OF THE TERRESTRIAL GLOBE.

By M. Arago.

Is there any variation in the thermometrical state of the globe in the long succession of ages? Do these changes affect the whole and entire mass of the globe; or, are they confined to its surface only? In either alternative, is there proof that the changes of temperature have been appreciable, within the period to which the records of history extends?

There are few inquiries which, for a considerable number of years, have more largely engaged the successful study of philosophers and geometricians than the above. They closely connect themselves with the future history of our race. They lead to plausible explanations of a great number of singular geological phenomena. They most naturally, therefore, attract attention. The last of them, concerning the change of climate, has at no distant period received much consideration, and this is the problem which, on the present occasion, we would desire to solve; at all events, to investigate, in all its bearings. Our object then will be, to present a sketch, as complete and elementary as possible, of the results which science has recorded up to the present time.

I. *At the origin of all things, the Earth was probably incandescent; and even now contains a large portion of its primitive heat.*

We shall have made a first step towards the demonstration of these two fundamental propositions, if we succeed in discovering whether, at the commencement, the globe were in a fluid or in a solid state.

If it had been solid when it began its rotatory motion, it would have retained the form it then possessed, in spite of its rotation; on the other supposition, it would have been the contrary. A fluid mass, in the end, necessarily assumes a figure of equilibrium, corresponding to the various forces to which it is subjected; or theory demonstrates that such a mass, at first homogeneous, must become flattened in the line of the axis of rotation, and bulge out at the equator; it gives the difference of the length of the two diameters; it shews that in the final state of equilibrium, the general figure of the mass is that of an ellipsoid; it also exhibits the modifications which may result, in physical hypotheses the most nearly allied, from a want of homogenousness in the liquid strata. Now, all the results of this calculation, whether we regard them in the general, or individually, agree in a most wonderful manner with the numerous measurements of the earth which have been made in either hemisphere. This result cannot, by possibility, be the effect of chance.

II. *The Earth, then, was at one time fluid.*

We must endeavour, then, to discover the cause of this original fluidity. It has just been stated that this cause was *fire;* but much is wanting ere unanimity be obtained on the point. The *Neptunists* will admit nothing but an aqueous fluidity. According to them, every terrestrial matter, however distinct in its properties, was originally dissolved in a menstruum; and the solid frame-work of the globe is formed by deposition or precipitation. The *Plutonists*, on the other hand, wholly reject the idea of solution. According to them, the original fluidity of the constituent principles of the globe, was the result of an exceedingly high temperature; and the surface has become solid by refrigeration.

The two schools, or sects we may rather call them, such has been their mutual ire, have contended with arguments but little decisive, taken from the phenomena of geology; but which have left the rigid inquirer in suspense. The true mode of terminating this debate, evidently was to examine if there existed in the interior of the earth, any circumstances which gave certain signs of the original heat invoked by the Plutonists. To this important problem, naturalists and geometricians have succeeded, by united efforts, in finding a satisfactory solution.

In every part of the globe, on descending to a certain depth, the thermometer ceases to exhibit either a diurnal or an annual variation. It invariably indicates the same degree, and the same fraction of a degree, during all the year, and during a succession of years. Such is the fact; and now, what is the theory? Let us suppose for a moment that the earth had received all its heat from the sun, then the calculation, *on this hypothesis*, would teach us, 1st, that at a certain depth the temperature would be invariable; and, 2d, that this solar temperature of the interior of the globe would change with the latitude. On these two points theory and observation agree. But, according to this theory, it must moreover be added, that in each climate, the invariable temperature of the strata should be the same at all depths, so long at least, as we do not descend to a very great extent, relatively to the earth's diameter. But every one knows that this is not the case. Observations made in a vast multitude of mines, and others made on the temperature of the water issuing from a great number of springs at different depths, agree in giving an increase of nearly 2° Fahr. for every sixty or ninety feet of descent. When a hypothesis leads to a result so completely in opposition to the facts, it is false, and ought unhesitatingly to be rejected. Thus, it is contrary to the truth, that the phenomena of the temperature of the successive strata of the earth, can be attributed to the sole action of the solar rays.

The solar action being thus rejected, the cause of the regular increase of caloric, observed universally, as we penetrate into the interior of the globe, can only be referred to an original heat belonging to the earth itself. The earth, as the Plutonists will have it, and Descartes and Leibnitz before them, the one as well as the others,—the earth, it must needs be avowed, even without any demonstrative proof, is now become expressly, *an* *incrusted star*, whose high temperature may be boldly invoked, whenever the explanation of geological phenomena shall require it!

III. *Is there any means of determining for how many centuries the Earth has been cooling down?*

In the mathematical theory of heat, there are many results which the present state of science allows us to pursue to their numerical applications. On the other hand, there are problems, whose solution have as yet been expressed only by general analytic formulæ. Among these latter there is a formula, which is intended to estimate the value of the cooling of the globe during the period of a hundred years, and in which THE NUMBER of centuries since the commencement of the cooling process bears a prominent part, assuming withal, that at this epoch a uniform temperature had pervaded the whole mass.

If the number of centuries were given, we could, from this formula, deduce the numerical value of the globe's loss of heat every hundred years; and inversely, from the quantity of the centenary refrigeration, when once known, we might, without difficulty, calculate the epoch at which refrigeration commenced. The question, then, so keenly disputed, concerning the antiquity of our globe, comprehending its period of incandescence, is thus brought to the determination of a thermometrical variation; which, moreover, on account of its extreme minuteness, must still be reserved for ages to come!

IV. *In two thousand years, the general temperature of the mass of the Earth has not varied the tenth part of a degree. The demonstration of this proposition is derived from the orbit of the moon.*

We have already admitted that the earth was at one time incandescent, and that its solid covering has been formed by refrigeration. It has moreover been proved that its heat, even at moderate depths, is still enormous. From this last circumstance, it follows that it must be still cooling. The only doubt that can be entertained is as to the extent. As, then, the title of this chapter indicates, we deduce, from the path of the moon, the proof, that in 2000 years, the mean temperature of the earth—

this temperature considered in the mass, and not at the surface only, has not varied a hundredth part of a degree!

Irrespective of the vast importance of this result, we have thought that curiosity would not be a little excited, in learning how these two phenomena, apparently so distinct,—how the heat of the earth, and the movement of a star, could mutually subserve to a counter reckoning of their phenomena. But to make this apparent, is an important object in the subsequent remarks. It is also hoped and anticipated, that we may be able to present an exact epitome of this difficult question to our readers, without the aid of any abstruse calculations.

Let us suppose then, that to each of the spokes of a common wheel, such, for example, as that used in grinding, some heavy body should be accurately fitted. Let us also suppose, that these weights could easily glide along the spokes, in such a manner that they might readily form a junction, whether it were near the axis of rotation, or towards the outer circumference of the wheel, or in any intermediate point.

With a correct notion of this apparatus, we can easily conceive that all the moving weights or masses, are placed, first near to the axle, and we may inquire what force, applied to the handle, will be necessary to give to the wheel the velocity of a revolution in a second.

After this first experiment, let us move along each spoke, from the centre to the circumference, the weight which is attached to it. After this, the wheel will weigh neither more nor less than it did before; and yet to make it again move round with the velocity of a revolution in a second, will require a much greater force.

After having a correct apprehension of the apparatus in these two conditions, it is scarcely possible that any one can question the accuracy of the result above alluded to. At all events, the simplest experiments can easily verify its truth.

Since then, for the revolving of a wheel of a given weight with a certain velocity, a force is required, greater in proportion as the elements of which the total weight is composed are removed from the centre, it is evident, for it is only the same result differently expressed, that, under the action of a determinate force, the motion of the wheel will become slower in proportion as the different parts of the mass shall elongate themselves from the axis of rotation.

There are few who do not know that heat dilates all known bodies, and that cold contracts them. The more then, that the wheel, of which we have been speaking, is heated, the more will it enlarge and extend itself; that is to say, the more will the material particles of which it is composed, elongate themselves from the centre of rotation. The contrary effect will manifest itself during the decrease of the temperature. It follows then, that under the action of the same force, a given wheel will revolve so much the more rapidly as it becomes colder, and so much the more slowly as it becomes hotter.

In machines intended for exact measurements, as for example in watches, the difference of the rate arising from the change in the dimensions of the wheel, consequent upon the natural variations of the atmospheric temperature, are so great, that they must needs be remedied.

The moving power of all watches is a steel-spring rolled upon itself, which incessantly acts upon the whole system of indented wheels, of which the watch is composed. It is to be observed, however, that these wheels have not an even continued motion; it is sometimes interrupted; the stopping of the seconds-hand upon each of the divisions of the dial plate, shews this. Well then, the interval which elapses between two of these consecutive stops, and consequently the duration of the second as told by the watch, (and on this depends the duration of the minutes and hours) is regulated by the time which a metallic wheel, called the balance wheel, occupies in making a turn upon itself. If the principles above laid down be true, then we may deduce from them, supposing the watch regulated to the ordinary temperature, that by heat the balance-wheel becoming larger, will oscillate slower,—that the second will be too long,—that the watch will get slow. By cold, on the other hand, the balance-wheel will go too rapidly, it will stop the wheel-work at intervals too near each other, the seconds will be too short, and the watch will go too fast.

These different results are confirmed by experience. Those watches in which no remedy is applied to the defects of the balance-wheel, by the help of an arrangement, the description of which would be misplaced here, go too slow in summer, and too quick in winter.

These details which we have been just perusing, would have been wholly superfluous, had we not imposed upon ourselves the rule to make no use, in this place, of the principles of mechanical reasoning, without having previously shewn how we could experimentally establish its correctness.

Whatever has hitherto been said concerning a flat wheel, may evidently be applied, word for word, to any body, of any other figure.

For example, let us conceive to ourselves a sphere having a rotatory motion upon itself, in virtue of some original impulse. If its dimensions increase, its rate of rotation will diminish; the sphere will occupy a longer time in making a complete revolution. On the contrary, if the sphere contracts, its velocity will increase; it will require less time for each revolution.

Our globe, then, what else is it than a sphere suspended in space, and daily revolving upon its centre, in virtue of an original impulse? It follows, then, that if its bulk be augmented, it ought, from day to day, to turn more slowly upon itself; and if its size be diminished, its movement should be accelerated.

The materials of which the earth is composed dilate by heat, and contract by cold. Those who think that the earth is cooling, by this very sentiment admit that its radius is diminishing —that its volume is becoming smaller and smaller. But we have just seen that the size cannot diminish without the rapidity of rotation being increased. The question, then, whether the earth, 2000 years ago, was at the same temperature that it is in 1834, resolves itself into this, Did the earth make a revolution 2000 years ago, in precisely the same time as it does now?

Under the former form, the question seems imperiously to require thermometrical results, of which the ancients had no idea. But, in the latter, we shall find, in the observations they have bequeathed us, the means of knowing if the duration of the earth's rotation is still the same.

What, in truth, is the duration of this rotation? It is nothing else than a certain *space* of time, which astronomers used in ancient times, as they still do in our own: it is, in a word, what they called the *sidereal day*. That there may be no doubt on the subject, it may be satisfactory to examine how this sidereal day is determined.

In every observatory there is a wall, carefully built and finished, or something equivalent, extending with the greatest accuracy from south to north. The astronomer who wishes to see if his pendulum is regulated by sidereal time, notes, with all possible precision, the moment when a star is seen along the length of the wall. The next day he repeats the observation upon the same star. If twenty-four hours, neither more nor less, have elapsed between the first and second observation, the pendulum is regulated. It is going too fast or too slow, when, between the two transits of the star in the line of the wall, the hands have marked more or less than twenty-four hours *.

The ancients must have regarded the sidereal day as the measure, in time, of the rotation of the celestial sphere, because they believed that the earth was immoveable. The moderns have demonstrated that the earth revolves; and hence, that when the star appears to place itself in the line of the meridian wall, it is in reality the wall which comes to meet the star. They were therefore inevitably led to see, in the sidereal day, the duration of the rotation of our globe.

We have thus brought the question of temperature, which we wish to solve, to a problem of the measure of time, because the ancients were not acquainted with the thermometer. Will any one ask, what has been gained by this substitution, seeing that the ancients no more possessed pendulums; or, at all events, none of them have come down to our times? True: but we

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* Respecting a pendulum regulated according to sidereal time, it ought to be noted, that such a pendulum, indicating that exactly twenty-four hours have elapsed between the two consecutive transits of a star by the meridian wall, would indicate $24^h\ 3'\ 56''$ during the solar day of mean time in common use. But this difference may be easily explained. Let it be supposed that the sun and a certain star are in the same region of the heaven; that is to say, let it be admitted, that, as on this day, the two luminaries pass at the same moment the line of the meridian wall. To-morrow, on the return of the star; in other words, when the sidereal day shall have been finished, the sun will no longer be in the same position as with the star, it will be more to the east; it will not arrive at the line of the wall, till after all the points of the arc by which it is removed, shall have passed the meridian. Now, the time required for the arc of the sun, so displaced, during a day, wholly to traverse the meridian, is according to its mean value, 3′ and 56″.

go on to shew, that for the determination of what has been the duration of the sidereal day for 2000 years, we have proofs infinitely superior to antique machines, whose correspondence and identity might be contested; and besides, that unless this duration were the same, the years must inevitably have become shorter.

The moon is not immoveable in space. She moves from west to east. It is from west to east that we see her successively traverse all the constellations of the zodiac.

The moon's movements have, in all times, attracted the especial attention of mankind; and they have been peculiarly solicitous to measure its velocity. But the measure of velocity implies the choice of an unit of time; and it will be readily admitted that this unit of time has been the sidereal day.

That the choice of the sidereal day as the unit of time, should furnish no objection in the problem concerning the velocity of the moon, it is necessary that the duration of this day, or, what is the same thing, it is necessary that the duration of the rotation of the earth, should be independent of the velocity of our satellite. But it is manifest that this independence exists. Even should the earth, on a sudden, cease to rotate upon its axis, the moon would nevertheless continue to traverse the constellations of the zodiac, as she at present does.

The Alexandrian school has left us observations, from which we can deduce, with the greatest accuracy, what, 2000 years ago, was the mean course which the moon ran during a sidereal day. The astronomy of the Arabians supplies us with elements for the determination of the same point, for the time of the Caliphs. And there is not a single catalogue of modern observations, where we do not find, for the present epoch, the value of the mean course of the moon during the duration of the sidereal day.

Well then, it happens that the arc described in a sidereal day by our satellite, proves to be *exactly the same*, whether we calculate by the Grecian observations, or by the Arabian, or by those of our own day *.

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* It is true that if we take the rough observations, the arcs described by the moon at the three distinct epochs, the Grecian, Arabian, and Modern, will not be equal. Since the time of the Chaldeans, the velocity of the moon

And this important result comprehends the solution of the question before us. Few words will be required to prove this.

The Alexandrian astronomy determined, by direct observations, the duration of *its* sidereal day, or of the rotation of the earth. It allowed the moon to progress exactly throughout this time, and noted the arc which it had run. The Arabian astronomers proceeded in exactly the same manner, and such precisely is the method still followed by ourselves. Thus each assumed the sidereal day of his own epoch. But since the moon, as we have seen, always moves with the same velocity, the progress which she makes must depend solely upon the duration of the time during which she pursues her course. If the sidereal day, at the time of Hipparcus, had been longer than it now is, the Greek astronomer would have observed the moon during *a longer time* than modern observers; the diurnal displacement of this luminary would have been greater than it now is; and its velocity would have appeared to us to have diminished. But the arc described in a day, is of precisely the same extent at all the epochs; therefore, including the most ancient observations, the word *sidereal day* has invariably designated the same equal space of time; and therefore (as the sidereal day and the duration of the earth's rotation are synonymous), during 2000 years the velocity of the rotation of our globe has continued the same; and, therefore, its volume is not changed; and, therefore, final-

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has, in reality, been continually increasing; but this increase is of the nature that, in astronomy, is denominated a *perturbation*. It depends on a diminution in the eccentricity of the ellipse which the earth annually takes round the sun. When this eccentricity, which hitherto has been diminishing, shall come to increase, the velocity of the moon will by degrees diminish, just as it has hitherto increased, and thus it will alternatively continue. The uniformity of the velocity then, announced above, is found only after correcting the lunar observations, in relation to the *perturbations* which the movement of the *translation* of the earth induces in the movements.

When it was recently said that the movement of the moon was independent of the movement of the earth, it is to be observed that this has reference only to our movement of rotation. If this remark was not supplied in this place, it might be supposed there was a contradiction, which, however, does not exist.

It may also be noted, that to Laplace belongs the merit of all these discoveries upon the moon's motion, and of their application to the inquiry of the invariability of the day, and of the temperature of the earth.

ly, the temperature, which could not undergo variations unless the earth's volume had corresponded to it, has remained stationary.

These deductions are all most simple, and it is to be hoped they will be apprehended without difficulty. It still remains that we should express, by numbers, the accuracy to which, by pursuing this theory, we may reach.

Let us suppose that the mean temperature of each one of the radii of the terrestrial globe had, in 2000 years, diminished 1° (centigrade). Let us assume glass as a standard for the measure of the general dilatability of the materials of which the earth is composed. We shall find this is nearly $\frac{1}{100000}$ to a degree. This diminution of 1° of temperature for each radius, would yield $\frac{1}{100000}$ of diminution in the dimensions of the sphere. But, at the beginning of this article, it was shewn how this diminution of the diameter must be followed by an increase of the velocity, and the principles of mechanics permit us to go even farther: they teach us, that for every $\frac{1}{100000}$ of diminution in the dimensions of the sphere, there should be a corresponding acceleration of $\frac{1}{50000}$ in the velocity. The sidereal day, then, should have fallen short of 86,400″ (the number of seconds of which it is composed), by its being divided by 50,000″, that is, by $1\frac{7}{10}''$. But the observations of the moon's movements prove, that, since the time of Hipparcus, the sidereal day has not varied even $\frac{1}{100}$ of 1″*, a quantity 170 times less than $1''\frac{7}{10}$.

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* Perhaps there might be incredulity as to this astonishing accuracy, if we were to say nothing of the methods by which it is reached. Suppose, that to satisfy ourselves of the invariability of the sidereal day, we take at each epoch, as a standard, the course which the moon pursues during *a single one* of these days, and that which a single direct observation can supply. To what degree of accuracy can we thus arrive? By using the best instruments which modern astronomers employ, the arc described by the moon in a sidereal day, could be measured almost to a second of a degree. The moon could not traverse the second of a degree in less than two seconds of sidereal time. Accordingly, if there be an error, for example, in excess, in the determination of the lunar movement of 1″, it is as if we made the sidereal day too long by 2″ of time, which is very far from the accuracy assumed in the text. Let us remark, then, that it is not from the observations of a single day, that we deduce the diurnal movement of the moon.

Suppose now, then, that we measure the arc described by the same luminary in *ten days*. This arc will be ten times longer than that which corresponded to a single day; but the uncertainty of the experimental conclusion, The change of temperature which we have just supposed in the terrestrial radius, was therefore 170 times greater than it is possible to admit, according to the observations of the duration of the sidereal day; and hence, in 2000 years, the mean temperature of the general mass of the earth has not varied $\frac{1}{170}$ of a degree of the centigrade thermometer.

We should add much, indeed, to the result of the uncertainty which yet remains concerning the dilatability of the materials which compose the globe, by multiplying it by 10, or, that we may have an even number, by 17. This would extend it to the 10th ($\frac{1}{10}$) of a degree, beyond which the mean temperature of the globe (the globe being always regarded in the mass, that is to say, both the exterior and the interior being comprehended) has assuredly not varied in the space of 2000 years.

V. ***Does the original heat of the globe, still so apparent at a certain depth, contribute, in a marked manner, to the actual temperature of the surface?***

Marian, Buffon, and Bailly, so far as France is concerned, estimated the heat which escaped from the interior of the earth at 29 times in summer, and 400 times in winter, the quantity that reached us from the sun. According to this view, the luminary which enlightens us would contribute but a very small share of that heat of which we feel the happy influence.

This idea has been developed, with much eloquence, in the *Mémoires de l'Académie*, in a paper upon *the Natural Epochs*

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will still be a simple second. In truth, it relates only either to the details gone through at the point of departure, or to those at the point of arrival. Every one, then, can comprehend that these operations must be identically the same at the two extremities of an arc, whatsoever be its length. When, to get the diurnal arc, we divide by 10, the corresponding arc of 10 days, this division will diminish the error in the whole arc in the ratio of 10 to 1; this error, therefore, will not be more than the $\frac{1}{10}$ of a second of a degree, corresponding to $\frac{2}{10}$ of a second of time.

Finally, if we measure the lunar arc described in 200 days, when we come to divide this total arc, which includes a great number of circumferences by 200, thus to procure the diurnal arc, corresponding to the medium of the interval of 200 days, the only doubtful second by which this whole arc will be affected, will become in the diurnal arc a 200th of a second of a degree, corresponding to a hundredth of a second of time.

These explications must suffice in elucidation of the astonishing accuracy assumed in the text.

by Buffon, and in Bailly's Letters to Voltaire upon the origin of Science, and upon the Atlantis; but the ingenious romance, to which it served as a basis, has been dissipated as a phantom before the severity of mathematical calculations. Fourier having discovered that the *excess* of the *total* temperature of the surface of the earth, over that which resulted from the sole action of the solar rays, has a necessary and determinate relation to the increase of temperatures at different depths, has been able to deduce from the experimental value of their increments, a numerical determination of the excess in dispute; that is to say, of the thermometrical effect which the central heat produces on the surface; but, instead of the large numbers given by Marian, Bailly, and Buffon, what, will it be supposed, is that given by the learned Secretary of the Academy? why, the thirtieth part of a degree!

The surface of the globe then, which, at the origin of things, was probably incandescent, has, in the course of ages, cooled down to such an extent, as scarcely to preserve a sensible trace of its primitive temperature. It is nevertheless true, that, at certain depths, the original heat is still prodigious. The lapse of time will probably lead to great changes in the interior temperatures; but, at the surface (and the phenomena here are the only ones which can affect or compromise the existence of living beings), all the changes are reduced to almost *the one-thirtieth of a degree.* The frightful congelation of the globe, then, the date of which was fixed by Buffon, to the time when the interior heat should be wholly dissipated, is nothing more than an idle dream.

VI. *Is the temperature of celestial space variable? And can this temperature become the cause of changes in terrestrial climates?*

Fourier has, within these few years, introduced into the theory of climates a consideration which had hitherto been entirely neglected, and of which naturalists, at least, have made no explicit mention. He has given a prominency to the part which the temperature of these celestial spaces, in which the planetary movements are effected, ought to play; in which, in particular, the earth annually describes its vast orbit around the sun.

In regarding mountains, even under the equator, covered with perpetual snows, and also the rapid diminution of the temperature of the atmosphere, as observed by aeronauts in their ascents, meteorologists had imagined, that in those regions whence the extreme rarity of the air will ever exclude man, in those beyond the atmosphere especially, there must ever reign immeasurable cold. It was not only by hundreds but by thousands of degrees, that they had undertaken to measure it. But all this was most foolish exaggeration. The hundreds and the thousands of degrees have descended, under the strict examination of Fourier, to some 50° or 60° cent. 50° or 60° cent. below zero is the temperature of those spaces in which the earth revolves from year to year; and such is the degree that the thermometer would indicate in the whole region occupied by our system, if the sun and the planets which accompany it were to cease to be.

Fourier has arrived at this conclusion, whilst he was inquiring what phenomena would be observed if the earth were placed in a space deprived of all heat. According to this hypothesis, he remarks, that the Polar Regions would be subjected to an extent of cold much greater than actual observation indicates. The alternating of day and night would produce most sudden effects, and of vast intensity, &c. &c.

It is most desirable, that the memoir in which the learned Secretary of the Academy must have recorded the proofs of these important propositions, should not be lost, and that it should be speedily presented to the public.

The *heat* of celestial spaces*, whatsoever may be its intensity, is probably owing to the radiation from all the bodies in the universe whose light reaches us. Many of these bodies have disappeared; others only present not unequivocal marks of fading; others, again, increase in splendour; but these are the rare exceptions. But as the total number of stars and nebulæ, that are visible with the aid of telescopes, certainly surpasses many thousands of millions, we have every reason to believe that, from this side at least, the inhabitants of the terrestrial globe have no serious alteration of climate to apprehend.

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* Let no one be surprised at the use of the term *heat*, in speaking of 50° or 60° cent. below zero. This is no more than the temperature that Captain Parry and Franklin have weathered in their voyages to the Polar Regions; and

VII. *Can the variations which certain astronomical elements undergo, sensibly modify terrestrial climates?*

There is upon the globe but a single region, where, allowance being made for the atmospherical refractions, the days and nights have at all times an equal duration. This region bears the name of the terrestrial equator.

Everywhere else except in the terrestrial equator, the days and nights have in general unequal lengths. At Paris, for example, on the 21st of June, the day consists of sixteen hours, and the night of eight. On the 21st of December, on the contrary, the day consists of eight, and the night of sixteen. The 20th and 21st of March, and the 22d and 23d of September, are the only epochs, when there, the days and nights consist *exactly* of the same number of hours. The latter dates (the 20th of march, and the 22d of September) have this peculiarity, that then there is in all places of the earth, from pole to pole, and from east to west, a perfect equality between the time of the sun's presence above the horizon, and below it.

It is not necessary that we should minutely have studied the difficult question of terrestrial temperatures, that we may in general comprehend that, under all latitudes, the epoch of long days and short nights shall prove a time of high temperature; and of long nights and short days, on the contrary, of low temperature; that, in short, the thermometric extremes shall in every place have an intimate and necessary connexion with days of a long and of a short duration. Every cause which reduces this difference will make the summers and the winters less unlike. It is not altogether so evident, that from the same cause will result a change in the mean temperatures; but a certain *equalization* of seasons will so clearly ensue, and will be so capable of every where modifying the phenomena of vegetation, that it would be useless to examine, whether, since the records of history, this equalization be the result of a change of the form and of the position of the solar orbit.

A circle which makes the entire circuit of the heaven, and

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this is no slight effect of heat, when compared to the hundreds and the thousands of degrees of cold which probably prevail in the space uninfluenced by hat cause with which M. Fourier is engaged.

which is called the celestial equator, separates the northern from the southern constellations. The nearer a constellation is to the southern pole the shorter, with us, is the time which elapses between its rising and its setting. The contrary happens in the opposite hemisphere: the constellations included in it, show themselves above our horizon a number of hours greater as they approximate the north. Lastly, the intermediate constellations, those which traverse the equator, are visible twelve successive hours, and disappear during the twelve following.

The sun in its apparent annual course, is found during six months, in the southern constellations, and the other six it is north of the equator. No one can doubt, that at each period of the year, the length of the day is precisely equal to the time which elapses between the rising and the setting of the constellation which the sun has then reached,—of which it seems to form a part, and with which it participates in the diurnal revolution of the heaven. The problem whether the winter-days, compared with the summer, are now more or less unequal than they were 2000 years ago, leads us to inquire if, in their excursions from the north to the south of the equator, the sun has always stopt at the same constellations, or better still, at the same stars. Mathematically speaking, it has not. Since the most ancient observations, the northern and southern excursions of this luminary have been confined within narrower and narrower limits. We add, however, at the same time, that the annual change is extremely minute: that the total, at the end of 2000 years, has scarcely amounted to a quarter of a degree;—that, in other terms, the sun in his southern excursion, for example, now stops previous to his commencing to mount towards the equator, when the inferior edge of his disk is arrived at the star which his centre attained at the commencement of this period of twenty centuries. A variation so completely trifling, evidently cannot have induced a change worthy of remark, either n comparative lengths of summer and winter days, or in the phenomena of agriculture.*

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* Geometricians have discovered that the variation which is observable in the enlargements of the annual oscillations of the sun, to the north and south of the equator, is Periodical;—that after having diminished for a certain number of centuries, these oscillations begin to increase again, and thus in indefinite succession, without ever surpassing the narrowest limits.

The sun is not always equally distant from the earth. At this time its least distance is observed in the first days of January, and the greatest, six months after, or in the first days of July. But, on the other hand, a time will come when the *minimum* will occur in July, and the *maximum* in January. Here then, this interesting question presents itself,—Should a summer, such as those we now have, in which the *maximum* corresponds to the solar distance, differ sensibly, from a summer with which the *minimum* of this distance should coincide?

At first sight every one probably would answer in the affirmative; for, between the *maximum* and the *minimum* of the sun's distance from the earth, there is a remarkable difference, a difference in round numbers of a thirtieth of the whole. Let, however, the consideration of the velocities be introduced into the problem, elements which cannot fairly be neglected, and the result will be on the side opposite to that we originally imagined.

The part of the orbit where the sun is found nearest the earth, is, at the same time, the point where the luminary moves most rapidly along. The demi-orbit, or in other words, the 180° comprehended betwixt the two equinoxes of spring-time and autumn, will then be traversed in the least possible time, when, in moving from the one of the extremities of this arc to the other, the sun shall pass, near the middle of this course of six months, at the point of the smallest distance. To resume—the hypothesis we have just adopted, would give, on account of the lesser distance, a spring-time and summer hotter than they are in our days; but on account of the greater rapidity, the sum of the two seasons would be shorter by about seven days. Thus then, all things considered, the compensation is mathematically exact. After this it is superfluous to add, that the point of the sun's orbit corresponding to the earth's least distance, changes very gradually; and that since the most distant periods, the luminary has always passed by this point, either at the end of autumn or beginning of winter.

We have thus seen that the changes which take place in the *position* of the solar orbit, have no power in modifying the climates of our globe. We may now inquire, if it be the same concerning the variations which this orbit experiences in its *form*.

The *apparent* orbit of the sun, that is to say, the *real* orbit of the earth, is truly an ellipse very little different from a circle. In this ellipse the great axis perpetually maintains the same length. The eccentricity, on the other hand, varies.

The immutability of the great axis of a planet implies, according to one of Kepler's laws, the immutability of the period of the revolution of this planet around the sun. Thus, whatsoever may be the changes in the eccentricity of the earth's ellipse, the length of the year will continue always the same.

According to this conclusion, the problem before us will come to this: Will the earth, considered as a whole, receive the same quantity of heat from the sun, whether it move round this luminary in 365¼ days in a perfect circle, or in an ellipse more or less elongated; always, however, having a greater axis equal to the diameter of the circle?

We may suspect that the answer to this question will be negative; that is to say, that the total quantity of heat received by our globe, will increase with the eccentricity of the ellipse, if in thought we will suddenly carry this eccentricity to the extreme limit;—if we will make the orbit so narrow, that the two sides will almost graze the surface of the sun; and thus make the earth touch that luminary twice a year. In fact, an exact calculation supplies the measure of the increase for every case; it teaches us, that the earth should annually receive from the sun, total quantities of heat, inversely proportional to the lesser axes of the elliptical orbits (with an invariable greater axis) in which we successively circulate.

At present the eccentricity of the terrestrial orbit diminishes; consequently the smaller axis increases; and hence the heat which we are receiving every succeeding year from the sun should be becoming feebler. But, in truth, this is nothing more than a mere abstraction: the variation of eccentricity going on so slowly, that it would require more than 10,000 years ere it would occasion a change that could be measured by the thermometer, in the temperature of the earth. When we ascend only to the remotest periods of history, the influence of this cause may then be wholly neglected.

Herschel, who has recently been occupying himself with this problem, in the hope of discovering the explanation of several geological phenomena, allows that the succession of ages might bring the eccentricity of the terrestrial orbit to the proportion of that of the planet Pallas, that is to say, to be the $\frac{2}{100}$ of a semi-greater axis. It is exceedingly improbable that in these periodical changes, the eccentricity of our orbit should ever experience such enormous variations; and, even then, these twenty-five hundredth parts ($\frac{25}{100}$), would not augment the *mean* annual solar radiation, except by about one hundredth part ($\frac{1}{100}$). To repeat, an eccentricity of $\frac{25}{100}$ would not alter in any appreciable manner the *mean* thermometrical state of the globe. It would only follow, that at an interval of six months, the greatest distances, and the least from the earth to the sun, which now differ scarcely a thirtieth ($\frac{1}{30}$), would then be in the ratio of 5 to 3. At comparative distances of 3 and 5, the illuminating and heating intensities of the sun, would be, between them, nearly as 3 to 1. Let us now, then, grant the intensity of *three* to the summer solstice,—in other words, let us place three suns over our heads in the months of July and August, and we shall form a tolerably correct idea of the excessive heat which we should experience *during certain days*, if the eccentricity of our orbit were twenty-five hundredths ($\frac{25}{100}$). I cannot, however, too often repeat, that such an eccentricity can probably never occur; and, at all events, we cannot find it, in going back upon the past, even as far as fifteen or twenty thousands of years, of our present epoch.

VII. *Of Terrestrial Climates, as they may be known from Observations made in different Ages.*

Thus, then, have we disencumbered, so far at least as it regards the phenomena which exhibit themselves at the surface, the problem of terrestrial temperatures, of many elements which have greatly complicated it. We have found that the central heat could not occasion any sensible variation of climate, since its *whole* effect at the surface, does not now exceed one-thirtieth ($\frac{1}{30}$) of a degree. The temperature of space, whatever doubts may yet remain as to the value which Fourier has assigned to it, must remain very nearly constant, if its cause, as every thing leads us to believe, is the stellary radiation. The changes of the form, and of the position, of the terrestrial orbit, are mathematically inoperative, or, at most, their influence is so minute that it is not indicated by the most delicate instruments. For the explanation of the changes of climates, then, there only remains to us, either the local circumstances, or some alteration in the heating or illuminating power of the sun. But of these two causes, we may continue to reject the last. And thus, in fact, all the changes would come to be attributed to agricultural operations, to the clearing of plains and mountains from wood, the draining of morasses, &c. &c.; provided we succeed in proving, that the climate has neither become hotter or colder in a spot, the physical aspect of which has not sensibly varied in a long course of ages.

Thus, at one swoop, to confine, for the whole earth, the variations of climates, past and future, within the limits of the naturally very narrow influences which the labour of man can effect, would be a meteorological result of the very last importance. We shall therefore, we trust, be pardoned, for the minute details into which we are about to enter. A great number of them, we are anxious to declare, have been brought together in the works of M. Schouw, a Danish traveller, alike distinguished for his botanical and his meteorological labours.

VIII. *The Mean Temperature of Palestine does not appear to have changed since the time of Moses.*

That the palm-tree may bear fruit, or, more accurately, that the date may ripen, a certain degree of mean temperature is, at *the least*, required. On the other hand, the vine cannot be cultivated with profit; it ceases to afford fruit fit for the manufacture of wine, so soon as this same mean temperature *exceeds* a point of the thermometer equally determinate. But the *lowest* thermometrical limit for the date, differs but very little from the *highest* thermometrical limit for the vine; if, then, we find, that at two different epochs, the date and the grape ripened *simultaneously* in any given place, we may affirm that, in the interval, the climate has not perceptibly changed. Let us now see how far this influence is supported by facts.

Jericho was called the town of palm-trees. The Bible alludes to the palm-trees of Deborah, situated between Rama and Bethel, of those skirting the banks of the Jordan, &c. The

Jews ate the dates, and prepared them as dry fruits. They also obtained from them a kind of honey, and a fermented liquor. The Hebrew money exhibits distinct representations of palm-trees covered with fruit. Pliny, Theophrastus, Tacitus, Josephus, Strabo, and others, have all made mention of woods of palm-trees in Palestine. There is no room, then, to doubt that this tree was cultivated, on a large scale, by the Jews.

In exactly the same way, we find that there are documents concerning the vine; and they shew that it was cultivated, not only that they might eat grapes, but also that they might make wine. Every one remembers the cluster which the messengers of Moses gathered in the land of Canaan, and which required two men to carry it. In twenty passages in the Bible, mention is made of the vineyards of Palestine; and the feast of tabernacles was celebrated after the vintage was passed. The book of Genesis speaks of the wines of Juda; and we know that the vine was not only cultivated in the northern and mountainous parts of the country, since frequent mention is made of the vines and wines of the *valley of Engaddi.* We may allude also to the testimony of Strabo and Diodorus, for both boast much of the vines of Juda. Finally, we may add that grapes are figured as a symbol on the Jewish money, quite as frequently as the palm.

To repeat then, it is well established that in the most ancient times, the palm and the vine were *simultaneously* cultivated in the Valleys of Palestine.

Let us now, then, examine what degree of heat the ripening of the date and of the grape requires.

At *Palermo,* whose mean temperature is nearly 62°.6′ Fahr. the date grows, but its fruit does not ripen.

At *Catania,* with a mean temperature of 64°.6′ and 66°.2′ the dates are not eatable.

At Algiers, whose mean temperature is about 70°, the dates ripen well. Nevertheless they are assuredly better in the interior of the country.

In reviewing these data, we may assert, that at Jerusalem, in whose environs the date was largely cultivated, at a time when the fruit of this tree served as the food of the population, the mean temperature was not under that of Algiers, where the date ripens and nothing more. This, then, would indicate the temperature of Jerusalem to be 70°, or somewhat more.

M. Leopold de Buch places the southern limit of the vine in the island of Ferro, one of the Canaries, the mean temperature of which must be between 69°.8′ and 71°.6′ Fahr.

At Cairo and its neighbourhood, with a mean temperature of 71°.6,′ we find here and there some vines cultivated in the gardens, but no vineyards properly so called.

At Busheir, in Persia, whose mean temperature does not certainly exceed 73°.4,′ it is impossible, according to Niebuhr, to cultivate the vine except in dry ditches, or in shady places out of the direct influence of the sun.

But we have seen that in Palestine, in the remotest times, the vine was, on the contrary, generally and largely cultivated, so that it is necessary to admit that the mean temperature of this country does not go beyond 71°.6. And the cultivation of the palm has just taught us, that we must not allow for this same temperature a number under 69°.8. And thus the simple phenomena of vegetation lead us to characterize, by a temperature of 71°, the climate of Palestine *at the time of Moses*, and it appears that any error can scarcely extend to an entire degree.

And what is the mean temperature of Palestine at the present day? Unfortunately, direct observations are awanting; but we can supply them by terms of comparison taken from Egypt.

The mean temperature of Cairo is 71°.6; Jerusalem lies 2° more to the north. Two degrees of latitude correspond, in these climates, to betwixt a degree and a degree and a half of Fahrenheit's scale. The mean temperature of Jerusalem then ought to be a little above 69°.8. For the more remote times, then, we find the two extremes 69°.8′ and 71°.6′, and for the mean 70°.8′.

All, then, leads to the conclusion that the climate of Palestine has not changed to any appreciable extent in the course of 3300 years.

The invariability of this particular climate may also be inferred, though with less certainty, from many other agricultural facts.

The culture of wheat, for example, would prove that the mean temperature does not exceed 75°.2′ or 77°. The balsam trees of Jericho, on the other hand, would mark from 69°.8′ to 71°.6 as the lowest limit.

The Jews of old celebrated the feast of Tabernacles, or of the vintage, in October. It is in the end of September or beginning of October, that they now gather the grapes in the neighbourhod of Jerusalem.

In ancient times, they had their harvest in Palestine, from the middle of April to the end of May. And travellers in our days have seen the barley perfectly yellow in the southern part of the same country in the middle of April. Near to Acre, it was ripe on the 13th of May. We also knew that in Egypt, where the temperature is higher, they now cut the barley at the end of April, or beginning of May.

The reason of our having brought together so many arguments, all bearing upon the same region of the globe, may be perceived, if it is considered that Palestine is one of the countries of the old world, which must least of any have experienced those peculiar modifications of climates, the cause of which we are seeking for in the general improvement of the country, and the agricultural labours of man. Hence, for it is well to repeat it, the invariability of the temperature of this country must lead to this conclusion, that thirty-three centuries have not induced any change in the illuminating and heating properties of the sun. But the demonstration of this proposition could not be supported by too many proofs, since it has been stated of the stars, we ought rather to say the far distant suns, that their light becomes dim, and terminates at length by a total disappearance.

IX. *Of the Climate of Europe in Ancient Times.*

The various memoranda which we have found in the most ancient authors, concerning the agricultural productions of Palestine, have a perfect agreement amongst themselves. The plants were accurately defined, and the localities were accurately mentioned, so that we obtained all desirable certainty. It will generally be supposed, that this plan of investigation, would be equally satisfactory concerning the ancient climate of Egypt. This, however, is not the case. And this not because there are no memorials; but because their discrepancies are so great, that we cannot safely draw any deduction from them. Take, for example, the vine. A passage from Herodotus will inform us, that the Egyptians did not cultivate it at all; while, at the same time, we shall find Athanæus boasting of the wines of Alexandria. Should we be solicitous of discovering the southern limits of its cultivation, we shall find in Theophrastus, express mention of vines growing in the immediate neighbourhood of Elephantina. But such information will always be useless, for the question about climate is to be elucidated, not by statements of the latitudes in which the vine grows, but by those in which it is incapable of supplying good wine. The documents concerning the palm-tree are not more satisfactory. According to Strabo, these trees were barren, or at least did not furnish good dates, at Alexandria and throughout the delta. And then again it is stated that the whole of the Lower Egypt was covered with them. It is wise, then, to leave such obscure passages, which often rested only on doubtful reports. Let us now proceed to the study of our own climes.

And here let it be again remarked, that we are about to occupy our attention with changes distinctly local, without pretending to extend even throughout a whole kingdom, that which may have happened at a particular part of it. Every other method of examination will assuredly fail of that precision, which, in the present time, we have a right to expect in a scientific discussion. Should it be wished, that we should also for a moment follow Daines Barrington, the Abbé Mann, and many other naturalists, in the efforts they have made to prove that the climate of the whole continent of Europe, and of some parts of Asia, has been considerably deteriorated in the course of ages? Should it be desired, that, after the example of these authors, we should proceed by taking the exceptions, and the extraordinary phenomena only? If so, analogous phenomena quite as extraordinary, and quite recent, present themselves in abundance.

We are told to read Diodorus Siculus, and thence learn that, in ancient times, the rivers throughout France were often frozen in winter, so that infantry and even cavalry, chariots, and the weightiest equipages crossed them without danger. So, too, according to Dion Cassius, Trajan's famous bridge across the

Danube was built to render the passage over this river easy during the winter, *when the cold did not freeze the waters.* Herodian has a narrative of the soldiers, instead of going with pitchers to seek for water on the banks of the Rhine, supplying themselves with axes, and, after cutting the ice, removing it to their camp.

And what are we to conclude from these passages? Nothing more than that, in the time of the Romans, the rivers of *France,* and the *Rhine,* and the *Danube,* were sometimes completely frozen over.

But now we shall present a table which will demonstrate, that, in far later times, these same rivers, on the one hand, and the Po, the Gulf of Venice, and even the Mediterranean itself, were frequently frozen.

In 860, The Adriatic and the Rhone were frozen.

NOTE.—The complete congelation of the Rhone, near to Arles, or in any other part of Provence, seems to require, according to observations made in 1776, a degree of cold of —0.4 Fahr.: In 1709, when the Gulf of Venice was frozen over, the thermometer in the city descended to 4° below zero. (Ac. des Sc. 1749.)

1133. The Po was frozen over from Cremona as far as the sea. The Rhone was crossed upon the ice. And the wine was congealed in the cellars. (This shows the thermometer must have been at zero.)

1216. The Po and Rhone frozen to a great depth. (Thermometer at least at zero.)

1234. The Po and Rhone again frozen. Loaded carriages crossed the Adriatic opposite to Venice. (Thermometer 4° below zero at least.)

1226. The Danube frozen over quite across, for a considerable time.

1290. Loaded carriages crossed the Rhine upon the ice opposite Brysach. The Categut was also completely frozen.

1302. The Rhone frozen. (At zero.)

1305. The Rhone, and all the other rivers in France frozen. (Papon. Hist. de Prov.)

1323. The Rhone frozen. Travellers on foot and horseback went on the ice from Denmark to Lubeck and to Dantzic.

1334. All the rivers of Italy and France were frozen.

1364. The Rhone was frozen at Arles, to a great depth; loaded carriages crossed it upon the ice. (Zero.) (Villani, quoted by Papon. iii. 210.)

1408. The Danube frozen throughout its whole course. The ice extended without interruption from Norway to Denmark. Carriages were driven upon the ice across the Seine. (Felibien, Description de Paris.)

1434. Frost began at Paris the last day of December 1433, and continued for three months all but nine days. It recommenced about the end of March, and continued till the 17th of April. (Felibien.) It snowed in Holland during 40 consecutive days. (Van Swinden from the Dutch Records.)

1460. The Danube continued frozen for two months. The Rhone also frozen.

1468. In Flanders, the soldiers' rations of wine were cut with the hatchet. (Philip de Comines.)

1493. The harbour of Genoa was frozen over the 25th and 26th December. (Papon. iv. 18.)

1507. The harbour of Marseilles was frozen throughout its whole extent. (This indicates the thermometer must have been below zero). On Epiphany day snow fell in the same city to the depth of three feet. (Papon. iv. 26.)

1545. In France, the wine was cut in the casks with sharp instruments. (Mezeray.)

1565. The Rhone frozen quite across, at Arles. (Zero.)

1568. The 11th December, carts crossed the Rhone on the ice. It did not break up till the 21st.

1570-71. From the end of November 1570, to the end of February 1571, the winter was so severe, that all the rivers, even those of Lanquedoc and Provence, were so frozen over as to bear loaded waggons. (Mezeray.)

1594. The sea was frozen at Marseilles and Venice, (at least 4° below zero.)

1603. Waggons passed the Rhone on the ice.

1621-22. The Venetian fleet was frozen in the Canals of Venice. (4° below zero.)

1638. The water of the harbour of Marseilles frozen round the shipping. (Papon. iv. 490.)

1655-56. The Seine frozen from the 8th to the 18th of December. It subsequently froze without interruption from the 29th December to the 18th January. A new frost returned a few days afterwards, and continued till March. (Bouillaud.)

1657-58. Uninterrupted frost at Paris from 24th December till 8th February. Betwixt the 24th December and 20th January, the frost was moderate: afterwards the cold was exceedingly intense. The Seine was universally frozen. The thaw of the 8th February did not continue. The frost returned on the 11th, and continued till the 18th. (Bouillaud.)

1658. This was the year Charles X., King of Sweden, crossed the Little Belt upon the ice, with his whole army, his artillery, waggon-train, baggage, &c.

1662-63. The frost continued at Paris from 5th December to 8th March. (Bouillaud.)

1676-77. Continued intense frost from the 2d December till 13th January. The Seine was frozen for 35 successive days. (Bouillaud.)

1684. The Thames was frozen over at London, the ice 11 inches thick. Carriages with company crossed it.

1709. The Adriatic and Mediterranean, at Genoa, Marseilles, &c. were frozen.

1716. The Thames frozen at London. A number of shops were established on it.

1726. Journeys were made from Copenhagen to Sweden in sledges.

1740. The Seine frozen across, with a temperature of 7° Fahrenheit; 1742, ditto, ditto, 14°; 1744, ditto, ditto, 16°; 1762, ditto, ditto, 16°; 1766, ditto, ditto, 16°; 1767, ditto, ditto, 3°; 1776, ditto, ditto, 10°; 1788, ditto, ditto, 8°; 1829, ditto, ditto, 6°.

After studying this long table, no one probably will discover, in the circumstance of the freezing of the rivers, as quoted by the ancients, any thing like *a proof* that the climate of Europe has deteriorated.

Nor will it be necessary now, to say much on the testimonies derived from the poets. Virgil, it is true, (Georg. III.) recommends "to sprinkle, under the sheep, in the fold, straw or fern, lest these delicate animals should be incommoded by the cold;" and, forthwith, there are authors who quote this passage, not very important, as an unanswerable proof, that the winters of ancient Italy were far more severe than we generally suppose. These exaggerations are already met, in our having shown, that in modern Italy, the Po, and the Adriatic, are frequently frozen. And can any thing else be demanded? We may add, that at Padua, not far from Virgil's Mantua, there fell, in January 1604, such a quantity of snow, that the roofs of many of the principal houses, being no longer able to support it, tumbled down; and that there, too, the wine was frozen in the cellars. At the side of these *facts*, well attested, what importance are we to attach to such meteorological documents as the litterings of straw or fern, recommended by the author of the Georgics?

The same poet somewhere says, that it has happened that the rivers of Calabria have frozen. How! oppose, it is exclaimed, such a fact as this? How longer deny that the winters in the south of Italy were formerly much colder than they are at present?

The objection is not so formidable as may be imagined. It may be remarked, in the *first* place, that the partial congelation of a river can never alone characterize a climate. There are several circumstances connected with atmospheric vicissitudes, which may cause incidentally to descend, upon any given point of the globe, very cold and dry strata from the higher regions of the air: and it would appear that the natural cold of these strata, with that from the abundant evaporation which their dryness produces, together with that which the night originates, from radiation, more especially if the atmosphere be perfectly serene, is perfectly sufficient to occasion the freezing of rivers in every region of the earth.* And thus, within these few years, we have learnt, if not without surprise, at least without esteeming the circumstance as altogether inexplicable, that in Africa, the water in the leathern bottles of Captain Clapperton was one night frozen, not far from Mourzouck, and in a plain but little elevated above the level of the sea. On the same accounts, meteorologists have not placed among the assertions unworthy of examination, what Abd-Allatif (See M. Silvestre de Lacy's translation, p. 505,) reports, viz. that in the year 829, when the Patriarch, James of Antioch, and Denys of Telmacher, went with the Caliph Mamoun into Egypt, they found the Nile frozen.

But previous to engaging in this discussion, we ought to have inquired if it be well established that the rivers in the south of Italy are never frozen in our times. Be this however as it may, to the testimony of Virgil might have been opposed a very decided passage from Theophrastus, whence we learn that at that remote period, the dwarf-palm (Chamærops humilis) covered a great extent of ground in Calabria. The vegetation of this shrub is quite compatible with a degree of cold, as in the kingdom of Valencia, where there are occasional incidental frosts of short duration; but it inevitably perishes when the cold is frequent, and sufficient to congeal the rivers.

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* These considerations serve to explain, 1*st*, how, in 1709, the Seine was not entirely frozen over at Paris, even between the bridges, whilst at Toulouse, the inhabitants were promenading on the Garonne, and in Languedoc they went on the ice from Cette to Boussigny and Balaruc; 2*dly*, how the maximum of cold occurred at Paris two days later than at Montpellier; 3*dly*, how, after a partial thaw, the cold returned to Montpellier sooner than to Paris; and, 4*thly*, how, in 1829, the cold was above a degree more intense at Toulouse than at Paris, which is situated 5¼° more to the north.

Strabo reports (lib. ii.) that at the embouchure of Palus-Meotis, the frost was so strong, that, in the winter, one of Mithridates' generals defeated the cavalry of the barbarians on precisely the same spot where, in summer, they were vanquished in a naval engagement.

This is one of those passages which the advocates for the general alteration of climates have most frequently brought forward. But it is of no great value; for Pallas, who resided long in the south of Russia, says, that, during even ordinary winters, the floating ice which comes down the Don not unfrequently covers the Strait of Zabache, but even a great portion of the Sea of Azoph: and that in some winters the loaded waggons pass without difficulty from bank to bank.

We may now return to the examination of the climate, so far as it relates to particular localities.

X. *Certain parts of Europe were not colder formerly than they are now.*

Strabo (lib. iv.) says, that the line of the Cevennes in Gallia Narbonensis was the northern limit beyond which the cold prevented the growth of the olive. The limit is still in the same position.

XI. *Certain parts of Europe were not formerly hotter than they are at present.*

The Greeks imported the date (Cordia myxa) from Persia into their own country. According to Theophrastus it yielded no fruit. This celebrated botanist adds, that in the Island of Cyprus, without completely ripening, it is eatable. The small quantity of heat, then, which this fruit in vain demands now-a-days to reach complete maturation in this same island, was also denied to it in remote antiquity.

XII. *Of the Climate in the neighbourhood of Rome.*

Theophrastus and Pliny report that the plains in the environs of Rome were covered with beech trees. The highest mean temperature in which these trees thrive does not exceed 50°; and the mean temperature of Rome is very nearly 60°. If, then, there be no mistake in these two authors, nor in the designation of the kind of tree, nor respecting the localities; if it be truly of the plains, and not the mountains, they intended to speak; then, the ancient climate of Rome, we perceive, should have been greatly ameliorated with the course of time. To a temperature but little inferior to that of Paris, must have succeeded one agreeing with that of Perpignan.

The idea that some error must have crept into the passages to which we have just alluded, is confirmed by the circumstance, that the latter of these authors, after having spoken of the beech, says also, that the laurel and the myrtle grow in the plain of Rome. But this supposes a mean temperature of at lest 55° 4′ or 57° 2′.* This brings us back to numbers, and though it be but to the lowest limits where these shrubs grow, yet it is much less removed from the real temperature. Let us, with Pliny, add, that, in his time, the laurel and the myrtle throve in Mid-Italy, even at some elevation upon the sides of the mountains. Let us also remark, according to the testimony of all travellers, that these plants do not, in these times, reach a higher habitat than 1200 feet; and from this coincidence, we may without hesitation conclude, that ancient Rome cannot possibly be colder than modern Rome.

Is it hotter? A statement of the younger Pliny seems to lead us to a negative response. In his letter to Apollinarius (lib. vi. let. 6) he says of a country-residence situated in Tuscany, "There are laurels. If they sometimes die, it is not more frequently than in the neighbourhood of Rome." Thus laurels were sometimes killed in the environs of Rome; hence the mean ordinary temperature of this city ought not to be much elevated above that which produces the death of laurels; and thus it could not much surpass that of 55°. The habitual vegetation of the laurel and myrtle gives us 57° at the least; and the occasional death of laurels has just given us a number somewhat above 55°. These two conclusions perfectly agree with the supposition of a constant mean temperature; for, to repeat, this temperature is now 60°.

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* These limits of temperature are true only in regard to continental climates. In islands, especially where the west winds, almost constant, passing over the sea, render the winter extremely mild, the myrtle can live with a mean temperature, far below 55° 4′. This plant, for example, thrives most wonderfully on the shores of Glenarm in Ireland, in the latitude of 55°. But this happens only because in such situations frost is scarcely known. It is because in them the winter is milder than in Italy. But what is gained in such situations in winter, is more than lost in summer. Thus, the grape does not ripen on the sides of Glenarm. I refer such as are solicitous of studying the differences which continental and maritime regions present as to the diffusion, throughout the season, of the same annual heat, to the excellent Memoir of M. de Humboldt on Isothermal Lines.

Varro places the vintage between the 21st of September and the 23d of October. The mean, at present, in the neighbourhood of Rome, is the 2d of October. The dates then, do not oppose the conclusion we have drawn from the culture of the myrtle and the laurel.

Is an additional proof required that, in ancient times, the plains of Romagna were not so cold as certain authors have alleged? They may be supplied from two interesting notices of Virgil, and Pliny the naturalist. In ascending the Apennines to a certain height above the sea, we find a great number of beautiful trees, which cannot now-a-days bear the high temperature of the lower regions, and amongst which I shall be satisfied with naming the pitch pine (*Pinus picea*) and the common fir. Well, then, in ancient times, these same trees did not descend to the plains. Both Virgil and Pliny expressly state that *the high mountains* are the sole localities in which they are to be found.

But we are anxious to acknowledge that, in this discussion, the data which it was necessary to employ, to a certain extent, have lacked that peculiar character, which recently allowed us, in reference to Palestine, to confine its ancient temperature between two numbers almost equal to one another. Nor need we much regret it. For, so far as it regards Rome, had we succeeded in establishing a difference of climate to the extent of 4° or 5°, we should not, from want of data, which would with certainty have informed us of the ancient state of the country compared with the present, have been able to discover the cause of the change.

XIII. *Change of the Climate of Tuscany.*

In the letter to Apollinarius, already referred to (p. 234.), the younger Pliny declares, that the climate of his residence in Tuscany was not favourable either to myrtles or olives. And yet

Pliny's house was not situated on high ground. He expressly says that it lay near the Apennines, at the foot of a hill not far from the Tiber. We must look to the inhabitants of the city of *Castello*, the ancient Tifernus, to decide whether, as I believe, the climate is now more temperate than in the time of Pliny. At all events, it may be well to notice if the surrounding mountains be still covered with lofty and ancient woods.

We shall now proceed to modern Tuscany.

As soon as Galileo had, towards the end of the sixteenth century, discovered the thermometer, the academicians *Del Cimento*, caused a great number of these instruments to be constructed perfectly similar to one another. These thermometers having been sent into various towns of Italy, were useful in supplying simultaneous meteorological observations. At this time the Grand Duke of Tuscany, Ferdinand II., employed the monks of the principal convents situated on his states, to aid in these interesting researches. The enormous mass of documents which had been thus collected were dispersed, at a time when, to obtain a Cardinal's hat, Prince Leopold de Medicis sacrificed the Academy del Cimento to the resentments of the Papal Court.

A few volumes only escaped, in a wonderful manner, from the vandalism of the agent of the Inquisition. Amongst them was a part of the observations made by the father Raineri, at the convent *Des Angeli* of Florence. These observations, compared with those of modern meteorologists, seem likely, on account of their antiquity, to throw light on the question of the change of climate. But, unfortunately, the thermometers of the academicians *Del Cimento* had no fixed term ; and different attempts, intended to establish the accordance of the degrees of this instrument, with those of Reaumur and Fahrenheit, only shewed that many more were still desiderated.

Such was the state of matters, when, in 1828, a box was discovered at Florence, which, among many other old instruments, contained a great number of the thermometers of the Academy del Cimento, divided into a scale of fifty parts. Mr Guillaume Libri to whom they were entrusted, and it would have been difficult to place them in better hands, began by ascertaining that they all agreed with each other. Then, with the help of more than 200 comparative observations, he compared their scale with the

the thermometers in present use. M. Libri found, for example, in this way, that the zero upon the *del Cimento* scale, corresponds to — 15° of Reaumur; and that the 50° of the former is identical with the 44° of the latter; and that at the freezing point the thermometer *del Cimento* stands at 30° 5′, &c. &c.

Provided with these results, M. Libri has taken the *maxima* and *minima* of each month during the sixteen years included in the long lost registers, and he has compared them with analogous results, which observations made since 1820 have afforded him, in the *Observatoire des Ecoles Pies* of Florence. This table has led to the important conclusion, that the clearing the wooded mountains, effected some sixty years ago, has not, in Tuscany, led, contrary to the very general opinion, to any sensible diminution of temperature. In the sixteenth century, the Apennines were covered with forests, and notwithstanding, in the space of fifteen years (from 1655 to 1670), Father Raineri saw his thermometer one year at 23°, another at 22°, a third at 15°, and finally, a fourth at 9° (Fahrenheit), excessive colds, which were not reached in the extraordinary winter of 1829–30.

In the table of M. Libri, the column of the *maxima* of temperature appears to offer a very important result. It would clearly appear from it, that, in the sixteenth century, the summers in Tuscany were much hotter than they now are. The observations of Raineri indeed present five *maxima* of 99°, two of 101°, and one of 102°. Between 1821 and 1830, the thermometer has only once stood at 99°. Thus the modification which the climate of Tuscany seems to have undergone is that of winters less cold, and summers less hot.

The learned geometrician, to whom we owe the discovery of the graduation of the thermometer *del Cimento*, would render an additional service to science, by examining if the observations of Raineri confirm the preceding result, not only as it regards the extreme degrees of heat and cold, but also in relation to the whole of the temperatures of each month, that is to say, to what is very properly denominated the mean temperatures.

XIV. *On the Changes in the Climate of France.*

The agricultural documents which will now be submitted to the attention of the reader, appear to establish that, in certain regions of France, *the summers are now become less hot than they formerly were*.

Many ancient families of the *Vivarais* have preserved, in the titles of their property, deeds which go as far back as the year 1561. These deeds indicate the existence of productive vines in grounds elevated more than 1800 feet above the level of the sea, and where now, not a single grape ripens, even in the most favourable exposures. To explain this fact, we must admit that in *Vivarais* the summers formerly were hotter than they now are.

This result is confirmed, so far as regards that part of the same country in which the vine is still cultivated, by a document equally demonstrative, but of a different nature.

Previous to the Revolution, there were in the *Vivarais* very many rents dependent upon the wine, and so settled in the sixteenth century. The great number of these rents were to be paid for, by the wine that was first drawn from the vat. For others again it was stipulated, that they might be taken from *the casks*, at the choice of the landlord. The time of this payment was fixed, making allowance for the corrected style, at about the 8th of October. The documents in question, prove that, on the 8th of October, the wine was in the casks, or at least in the vat, and at the point of being drawn off. Now the *minimum* of time that wine is left in the vat ere it is drawn off, is eight days. In the sixteenth century, then, the vintage must have been finished in the *Vivarais* in the latter days of September. Now, it is not finished till between the 8th and 20th of October. An inhabitant of the country has affirmed that he never saw it commence before the 4th of October.

These documents are silent as to the duration and the severity of the winters; but they appear to establish that, in the sixteenth century, at the 45° of latitude, and on the banks of the Rhone, the summers must have been hotter than they now are.

We read in the history of Macon, that, in 1552 or 1553, the Hugonots retired to Lancie, a village situated close to this town, and that there they drank the *muscat* wine of the country. But the muscat-grape will not now ripen in this district, in such a way that wine may be made from it.

The Emperor Julian had *vin de Surêne* at his table. The reputation of this wine is now proverbial; why, we need not add. But we must not attach more importance to this circumstance than it merits. The *quality* of a wine depends too much on the nature of the plant, and the care of the husbandman, to furnish satisfactory arguments on the question of the changes in climate.*

We find in an old charter quoted by M. Capefigue, that Philippe-Auguste (about the commencement of the thirteenth century), wishing to choose amongst all the wines of Europe the one which he would habitually drink, the vine-dressers *d'Etampes* and of *Beauvais* presented themselves among the general concourse. It is true, the document adds, that they were rejected; but can we conceive they would have had the impudence to come forward, if their wines had been as little potable then as all those of the department *de l'Oise* now-a-days are? This department at present gives, in France, the northern limit for the cultivation of the vine. In the public accounts given in, of the administration of indirect taxes for 1830, it is, in effect, stated, that the vine is not cultivated in the department *de la Somme.* Now, it is not in a region where the culture is scarcely possible that we seek for products of the best quality.

When the Emperor Probus (obit. A.D. 282,) permitted the Spaniards and the French to plant the vine, he conferred the same favour on the inhabitants of England. The favour would have been derision if, in these times, the vine did not thrive on the other side of the channel, (La Manche.)

Old chronicles also inform us, that, at a remote period, the vine was cultivated in the open fields in a great part of England, and that the wine was regularly manufactured. But now, the most assiduous attention, with a southern exposure, and complete defence from cold winds, are scarcely sufficient to bring very small grapes to complete maturity.

Here, then, it would appear, that there is evidence sufficient to convince the most incredulous, that, in the course of ages, the summers have lost, both in France and England, an appreciable part of their temperature.

It remains for us to inquire what is the cause of this unpleasant phenomenon,

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* The *quality* of a wine, and this must not be overlooked, depends very much on taste;—*good wine* of 1550 might be reckoned *very bad* in 1834.

XV. *Conjectural causes of the increasing coldness of the Summers of France and England.*

This increasing coldness is evidently not in the sun; and the constancy of the temperature of Palestine is a proof of it. Some naturalists think it may be found in an unusual extension of the ice-plains of the north pole; in a general movement which, after having carried them many degrees towards the south, has fixed them to the coast of Greenland.

It is certain that the eastern coast of Greenland was free of ice when it was discovered towards the end of the tenth century, by an Icelandic navigator; that the Norwegians established themselves there; that, in 1120, the colony was numerous and flourishing, and that it maintained a considerable commerce with Norway and Iceland. It is also known that, in 1408, when Andrew, the Bishop, the 17th in succession since the colonization, went to take possession of his chair, he found the coast entirely blocked up with the ice, and could not land. This state of things continued, with some variations, up to the year 1813 or 1814. At that time there was an immense breaking up, and the east coast of Greenland once more became free. The deterioration, then, of the climates of Europe, it may be held, was owing to the permanent existence of a vast plain of ice, which had extended itself in latitude from Cape Farewell to the polar circle.

But this explication will be completely upset, by our considering that the document already adduced to prove that, in the *Vivarais* and in *Bourgogne*, the heat at a former period was great; and this about a century and a half posterior to the formation of the icy plain on the coast of Greenland. It may be added, that the entire breaking up which the ice underwent in 1814, has produced in our climates, neither those *conspicuous changes* which agricultural phenomena might reveal to every one, nor even any of those slighter modifications, which the delicate instruments of meteorologists might indicate to them.

Let us now, then, consider if the cause of the variation in our climates is not much nearer to us; and, if it does not depend solely on those operations which the wants and caprices of an unceasingly increasing population have produced on a thousand points of territory.

Ancient France, contrasted with France as it now is, presented an incomparably greater extent of forests; mountains nearly wholly covered with wood; lakes, and ponds, and morasses, without number; rivers without any artificial embankment to prevent their overflow, and immense districts, which the hand of the husbandman had never touched. Accordingly, the clearing away of the vast forests, and the opening of extensive glades in those that remain; the nearly complete removal of all stagnant waters, and the cultivation of extensive plains, which thus are made to resemble the *stepes* of Asia and America—these are among the principal modifications to which the fair face of France has been subjected, in an interval of some hundreds of years. But there is another country which is undergoing these same modifications at the present day. They are there progressing under the observation of an enlightened population; they are advancing with astonishing rapidity; and they ought, in some degree, suddenly to produce the meteorological alterations which many ages have scarcely rendered apparent in our old continent. This country is North America. Let us see, then, how clearing the country affects the climate there. The results may evidently be applied to the ancient condition of our own countries, and we shall find that we may thus dispense with *a priori* considerations, which, in a subject so complicated, would probably have misled us.

But over the whole extent of North America, it is universally admitted that the clearing of the country has modified the climate; that this modification becomes every day more manifest; that the winters are now less severe, and the summers less hot; in other words, that the extreme of the temperatures observed in January and July, annually approach to each other.

Let us now compare these results with those which follow from the preceding discussion. As to Florence their identity was most conspicuous. In the middle and north of France, we see, like the Americans, that the summers are becoming colder. It is very probable too, that, according to the general opinion, the winters were formerly colder. But we have not found that this has been altogether *proved*. At all events, however, it must be allowed that nothing contradicts the opinion that, in Europe, the change in climate may be exclusively attributed to the agricultural improvement of the country.

The Americans have also noticed a marked alteration in the prevailing winds upon their coasts. (See the Works of Williams and Jefferson.) The former predominance of the west winds appears to be diminishing.* The east wind, besides becoming more frequent, is by degrees, penetrating further and further into the country.

The effect of clearing, then, in the United States seems to have been more moderate colds, and more moderate heats. But this says nothing as to whether its *mean temperature* has been altered. The improvement during the winter might then compensate for the loss during the summer. It is, however, probable that this compensation has no existence; for, amidst the multitude of important results which M. Boussingault has collected during his residence in South America, there is a table of observations of the *mean* temperatures of the equatorial zone, where we observe, without exception, that the lowest numbers correspond with the most wooded regions. It is, therefore, probable, to use an expression of Buffon's, that, at the same time, the American climate has become less *excessive*, its mean temperature has increased.

It may here, perhaps, be expedient to add a word or two, with the view of meeting the difficulties of those naturalists who refuse to recognise in the operations of human industry, in those labours which, it must be admitted, scarcely penetrate the very surface of our globe, a cause adequate to the production of a visible change of climate. We shall here, however, attempt nothing more, than to point out some localities where, on account of the favourable position of a hill in relation to the predominant winds, or of some undulation in the ground, or some other circumstances so little remarkable that they can scarcely be noticed, a superior climate is enjoyed.

Middlebourg, whose latitude is nearly a degree *less* than that of Amsterdam, ought to have a mean temperature *one degree greater*. In truth, however, it is fully *four degrees less*.

The town of Brussels itself has not a mean temperature so high as that of Amsterdam, though it be a degree and a half more to the south.

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* If any one doubts the immense predominancy of the west winds on the Atlantic, the following fact may be adduced as conclusive.

Taking the mean of six years, the Liverpool packets which sail from east to west, that is, to New York, employ in their voyage . - - - 40 days.

The same ships occupy in returning to Liverpool - - - - 23 days.

In Devonshire, the town of Salcombe enjoys a climate which has procured for it the appellation of the Montpellier of the North.

Marseilles is more than a degree to the south of Genoa; the mean temperature of this latter, then, ought to be a degree of Fahrenheit's scale less than that of Marseilles,—on the contrary, however, it is two degrees higher.

We ought not to be surprised that Marseilles, a maritime town, should have a climate warmer than that of Avignon, situated somewhat more to the north, and in the interior of the country, —that the winters there should be sensibly more cold, and the summers sensibly hotter; but let us here ask, what is the cause that the mean temperature of Marseilles is less than that of Avignon?

Rome and Perpignan have exactly the same mean temperature, although the former place is a degree more southerly. If the cause of this were demanded, allusion would probably be made to the Apennines; but then, Perpignan is at the foot of the Pyrenees.

But we shall not push such statements further. Plausible explanations might be offered of many of the anomalies, founded on the particular configuration of the countries in which these towns are situated, &c. &c. But this is not the question. What we have stated, and we still maintain, is, that the alleged causes of these anomalies are not remarkable; that an attentive examination of the localities would scarcely enable us to predicate what would be their effect, or in what way they would operate.

And this finishes the task we had undertaken. We shall conclude this long article, by briefly examining whether the mean temperature of Paris has, in our days, undergone any change.

Nothing, at first sight, can appear more simple than this question. The temperature *des souterrains*, which are not at all deep, and into which the external air has not free access, not only does not vary, but, moreover, it is equal to the mean temperature of the external atmosphere at the surface. This is the character of the excavations under the *Observatory* at Paris. They are about 90 feet deep. Thermometrical observations have been made there for a century and a half, and all that seems necessary is to examine these observations. But, without going back to the older instruments, for their graduation is not now well known, it must be remarked that a recent discovery has rendered the solution of the problem not a little difficult. It is now proved, that in the course of time almost all thermometers become untrue. The point indicative of the freezing of water mounts upwards along the scale, as if the bulb contracted upon itself. The thermometer will thus then come to indicate 33° or 34°, when it ought to indicate 32°, and 35°, instead of 34°, &c. &c. The error is sometimes found to extend to 3°. The numerous observations, then, made in *les souterrains* of the observatory, at a time when it was not known that thermometers must be corrected, are as if they had not been. Two observations, however, have been found, but two only, from which something may be extracted. They go back as far as February 1776. They were taken by Messier, with a thermometer, constructed under his own inspection, and verified by himself *a few days before.* These two observations, perfectly agreeing, give 11°,8 centigrade, (about 53° Fahr.) And in 1826, half a century afterwards, it has been found precisely the same. We shall suppose, now, that in the observations of Messier, on account of the smallness of the scale of his thermometer, there was an uncertainty to the extent of *one-twentieth* of a degree. These two temperatures of 1776 and 1826, which have appeared to us equal, would then have differed to this extent. But one-twentieth ($\frac{1}{20}$) in fifty years is $\frac{1}{10}$ in a hundred, *which would yield only one entire degree of variation in a thousand years.*

The two epochs which we have compared comprehend betwixt them a period during which some portions of France have been deprived of their woods to a very great extent. The mean temperature of Paris, however, seems to have undergone no appreciable change.

We have taken the observations of 1826 as the term of comparison, that we might thus have a round number of fifty years. Had we pushed the comparison farther, brought it down to 1833, we should have found that observation yielded 7 centiemes of a degree (.07) more. Thus, instead of a reduction of the temperature, we should have found a slight increase. But we should require to wait some additional three or four years, before we can affirm with certainty that this slight difference is not owing to some irregular and accidental oscillation.

SOME EXPERIMENTS AND OBSERVATIONS ON THE COMBINATIONS OF CARBONIC ACID AND AMMONIA. *By JOHN DAVY, M.D., F.R.S. Communicated by the Author.*

In referring to the note-books of my brother the late Sir Humphry Davy, for the purpose of drawing up a memoir of his life, I have found an account of some experiments, which he made in 1799 at Bristol, on the salts of ammonia, and more especially on the carbonates. He then ascertained that the common subcarbonate (the sesquicarbonate) is partially decomposed by heat, and that a portion of its carbonic acid is disengaged; and he believed, that when it is sublimed a new salt is formed, more volatile than any known carbonate, containing a larger proportion of ammonia, of most pungent ammoniacal odour, and deliquescent. The only published notice of these experiments, occurs in his researches on the nitrous oxide, introduced incidentally in connection with the analysis of the nitrate of ammonia. He says, "In making the analysis of the carbonate of ammonia, I discovered that there existed many varieties of this salt, containing very different proportions of carbonic acid, alkali, and water; the carbonic acid being superabundant in it, in proportion as the temperature of its formation was low, and the alkali in proportion as it was high; and not only that a different salt was formed at every different temperature, but, likewise, that the difference was so great that the carbonate of ammonia formed at 300°, contained more than 50 per cent. alkali, whilst that produced at 60° contained only 20 [*]."

It appears from a note to this paragraph, that it was his intention to have given a particular account of these experiments, but I cannot find that he did. Other more interesting and important inquiries, it may be inferred, diverted him from his purpose.

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[*] "Researches, chemical and philosophical, chiefly concerning nitrous oxide, &c." p. 76.

Since that time the carbonates of ammonia have been ably investigated, in relation to composition, by several chemists, especially by M. Gay Lussac, Dr Ure, and Mr Richard Phillips. These gentlemen have established, in a satisfactory manner, the existence of three definite compounds, namely, carbonate, sesquicarbonate, and bicarbonate. Whether another compound, such as my brother supposes he had obtained, is capable of being formed, is still undecided. To endeavour to determine this question, I have gradually been led into a set of experiments, concerning the combinations of carbonic acid and ammonia generally, much more extensive than I had at first contemplated, the results of which I now beg leave to communicate,—not, indeed, in the order in which they were obtained, but in that in which they can be most easily and briefly given.

1. *On the direct combination of Carbonic Acid and Ammonia.*

Since the publication of M. Gay Lussac's very original paper on the combination of gases in definite volumes, in the year 1808 *, it has generally been admitted, that carbonic acid gas, in uniting with ammoniacal gas, condenses of the latter exactly twice its volume; and, further, that this acid and alkali are not capable of uniting in any other proportions, without the intervention of water †.

Not having met with any account of the repetition of the experiment on the direct combination of the two gases, and considering it desirable that every fundamental experiment should be repeated, I have been induced to make it for my own satisfaction, using every precaution possible to insure accuracy. The glass tubes for transferring the gases, and for their combination, were carefully graduated, and scrupulously cleaned and dried ‡. The mercury of the trough, over which the combination was made, was heated twelve hours before, above the boiling point of water, and the tubes were filled with it whilst still warm *; and the gases of known purity, collected in a separate mercurial trough, were dried previous to their combination,—the alkaline gas by means of potassa (kali purum of the shops), the acid gas by chloride of calcium †.

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* Memoires d'Arcueil, tom. ii.

† Some chemists, I perceive, allude in their writings to an anhydrous bicarbonate of ammonia; but it would appear only hypothetically.

‡ The tube for combination, was of capacity 5¼ cubic inches, about 10 inches long, with a funnel-shaped mouth graduated to eighths of a cubic inch.

I do not consider it necessary to enter into a minute detail of the experiments made with these precautions. The results generally were amply confirmatory of M. Gay Lussac's accuracy, indeed even beyond my expectation, reflecting that the results of the best experiments are only approximations to absolute truth. Thus, in one instance in which $1\frac{1}{2}$ cubic inch of ammonia was condensed by carbonic acid added in excess, the residual excess of the acid gas did not differ from what might have been expected according to theory, more than the $\frac{1}{64}$th of a cubic inch; nor was the difference greater in another instance, in which 2 cubic inches of carbonic acid gas were condensed by ammonia in excess.

As a farther test of accuracy, I may mention, that the salt formed was exposed to the action of muriatic acid gas, and that, as might be expected, this gas, in decomposing the carbonate, disengaged exactly half its own volume of carbonic acid gas; I say exactly, for when $1\frac{1}{4}$th cubic inch was used, the gas evolved from the salt in excess was, as near as possible, one-half, after making allowance for a certain impurity in the muriatic acid gas employed‡.

The tubes for transference were similarly graduated and narrower. They were cleaned by washing them, first with dilute muriatic acid, and afterwards with distilled water; and they were dried by means of fine warm cambric.

* The trough had a cylindrical cistern attached to its bottom, to receive the tube in which the gases were mixed, so that no calculation was required for the effect of diminished pressure.

† The carbonic acid was obtained from Carrara marble, by means of dilute muriatic acid. Its degree of purity was ascertained, by introducing into it a fillet of paper moistened with two or three drops of a strong solution of potash over mercury, in a tube of the capacity of $\frac{1}{2}$ cubic inch, divided into 50 parts. The ammoniacal gas obtained from salammoniac by quicklime, was tried in the same way, by a fillet moistened with water.

‡ The muriatic acid gas was tested in the same manner as the ammoniacal gas, and just *before* the experiment. This, I hardly need mention, is necessary, as it is pretty rapidly decomposed when kept over mercury.

These trials were made at the ordinary temperature of the atmosphere, which at the time was pretty constant at 60°.

Though the salt formed by the direct union of carbonic acid and ammonia possesses singularly alkaline properties, and is as far as possible from being neutralised, yet it has very properly been called carbonate of ammonia, and been considered as composed of one proportion of each of its elements; and, I may remark, that, in accordance with this, it is decomposed by a solution of the neutral muriate of lime without effervescence, producing a neutral mixture.

2. *On the Sesquicarbonate of Ammonia.*

I apply the term Sesquicarbonate, after Mr Richard Phillips, to that salt of ammonia commonly called the subcarbonate, which is obtained by the mutual decomposition, by means of heat, of carbonate of lime and salammoniac.

The results of my experiments on the analysis of this salt, agree, as nearly as might be expected, with those of Dr Ure[*] and of Mr Richard Phillips[†], according to which the sesquicarbonate may be considered as composed of one proportion of ammonia, of one and a half of carbonic acid, and of one of water.

The method of analysis which I have applied to this salt, and also to the neutral or bicarbonate, is different from those employed by these gentlemen. It is briefly as follows. The proportion of carbonic acid has been ascertained by decomposing the salt[‡] over mercury in a graduated tube, by means of dilute muriatic acid in excess, making due allowance for the gas absorbed by the liquid[§]. The proportion of ammonia has been learned indirectly, by decomposing the salt by means of dilute muriatic acid in excess, driving off the excess of acid by exposure of the muriate, in the capsule in which it has been formed, to the temperature of boiling water; dissolving this salt in distilled water, and precipitating by nitrate of silver *. I may remark, that I had previously ascertained, by a very careful trial, that salammoniac, when exposed for several hours on a steam-bath, appears to suffer no more loss than may be attributed to the dissipation of a very little adhering or hygrometric water †; and, also, that, at the same temperature, after two or three hours' exposure of the salt, all excess of acid is entirely dissipated ‡.

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[*] Annals of Philosophy, vol. x. p. 203.

[†] Quarterly Journal of Science, vii. 294.

[‡] The salt is conveniently introduced wrapped in fine bibulous paper.

[§] Strong muriatic acid is capable of absorbing about half its volume of carbonic acid gas at ordinary temperatures; when moderately diluted with water, its power of absorption is much the same; and it is not sensibly altered by the addition of a little muriate of ammonia.

To shew the degree of accuracy this method admits of, applied to minute quantities, I may mention the results of two analytical trials which I made (and they were not the only ones) before I gave it my confidence; one on the proportion of carbonic acid in carbonate of lime, which may be considered as pretty well determined; the other, on the composition of salammoniac.

A minute fragment of pure Carrara marble, equal to 1 grain

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* I had recourse to this method, from not obtaining uniform results when I attempted to ascertain the exact proportion of ammonia, either by the quantity of acid, of a given strength, required to neutralize the alkali, or from the volume of gas disengaged decomposing the salt by quicklime, according to Dr Ure's plan; or, after Mr Richard Philips', by the quantity of carbonate of lime precipitated by the salt from the neutral muriate of lime. This last mentioned method, in my hands, has proved the least satisfactory of any that I have tried, the variations of results having been very great, according to incidental circumstances of temperature and of strength of solution, &c. The effect of temperature is most remarkable. It is necessary to employ heat, to separate the whole of the carbonic acid gas, and insure the precipitation of the carbonate of lime. The difficulty is to effect this without a loss of ammonia; for, if either the sesquicarbonate or bicarbonate in solution is poured into a hot neutral solution of muriate of lime, at 170° and upwards, there is a strong ammoniacal odour produced, at the same time that there is a violent effervescence from carbonic acid gas liberated, and a rapid precipitation of carbonate of lime; or, taking the reverse, if a mixture of carbonate of lime and solution of muriate of ammonia is heated to the same degree, a mutual decomposition of the two commences, as is indicated by the odour of the volatile alkali which is perceived.

† 5 grains of salammoniac from a translucent mass, reduced to powder, after exposure on the steam-bath for five hours, were reduced to 4.98 grains. When first placed on the bath, a faint peculiar odour was perceptible on very near approach, as if an extremely minute quantity of the salt was raised with the very diluted aqueous vapour.

‡ 5 grains of the same salt were dissolved in very dilute muriatic acid, slowly evaporated to dryness, and then kept on the steam-bath for about two hours after acid fumes had ceased to be perceived. The loss in this instance was precisely the same as in the first, namely .02 grains.

carefully weighed in a balance, which is distinctly affected by $\frac{1}{100}$ of a grain, was introduced into a tube over mercury, containing $\frac{5}{100}$ cubic inch of dilute muriatic acid. The gas disengaged when the marble was quite dissolved, was 89 cubic inches, or, allowing for absorption, by the acid, about .92. Now considering carbonate of lime as composed of 26.5 lime, and 20.7 carbonic acid; and 100 inches of the acid gas to weigh 47.469 grains, one grain of the marble ought to yield .927 cubic inch, a difference not appreciable between the estimate and the result.[*]

Five grains of salammoniac, part of a translucent mass, precipitated by nitrate of silver, afforded 13 grains of chloride of silver well washed, and perfectly dried, at a temperature above 212°[†]. Now, considering the proportion in which silver combines to be 108, that of muriatic acid 36.45, and that of ammonia 17, according to Dr Turner's estimates of the equivalents of silver, chlorine, and azote[‡]; and admitting that muriatic acid gas and ammonia combine in equal volumes to form muriate of ammonia, and that 100 cubic inches of ammonia weigh 18.3837 grs. at 60°, and 100 of muriatic acid gas 40.0121 grs.; it may be inferred from this result that Salammoniac is composed of

30.70	Ammonia;
66.86	Muriatic Acid;[‡]

or of

17 Ammonia	(1 proportion	17)
37 Muriatic Acid	(1	36.45.)

an approximation this as great as might be expected between an experimental result and an estimate; and agreeing exactly with another estimate, founded on the weight of the gases, according to which, the equivalents of ammonia and muriatic acid are 17 and 37. *

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[*] The barometer and thermometer at the time, were close on 30° and 60°. The power of saturation of the dilute acid was ascertained by experiment.

[†] The same result was obtained in three different experiments.

[‡] London and Edinburgh Philosophical Magazine and Journal, for August 1832, p. 110.

[‡] The loss 2.46 grs. may be considered in part owing to hygrometrical water, which, according to experiments, is .7 per cent.; and, in part, to the manipulations and the data for calculation derived from experiments.

I have said that the results of my analyses of the sesquicarbonate of ammonia, agree as nearly as might be expected with those of Dr Ure, and of Mr Richard Philips. They are exhibited in the following table:—

Exper.	Carbonic Acid Gas.	Chloride of Silver.
1	1.15 cubic inch.	11.6 grains.
2	1.16	11.
3	1.16	11.8
4	1.15	

The gas was procured from 1 grain of the salt, allowance having been made for absorption by the acid; and the chloride was obtained from 5 grains. In each instance, the salt was taken from a compact translucent mass, that had undergone no change from exposure to the air.

Calculating on these results, or 1.15 cubic inch of carbonic acid gas at 60° (the minimum quantity) for 1 grain, or 115 for 100 grains, and on the mean of the quantities of chloride of silver, namely 11.74 grs. or 234.8 per cent. it follows, on the

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* I may mention, that I have endeavoured to ascertain directly the proportion of muriatic acid in salammoniac, by decomposing it in a graduated tube over mercury, by sulphuric acid; but, that I have not been able to obtain perfectly satisfactory results, owing to the power of absorbing muriatic acid gas possessed by sulphuric acid. Sulphuric acid, of specific gravity, 1.85, absorbs very nearly its own volume of muriatic acid gas, at 60°; the absorption takes place slowly, even when agitation is employed. The acid gas imparts to the suphuric acid its peculiar odour; and, if ammonia is brought near, fumes may be seen on the exposure of the acid to the open air.

On the supposition, that salammoniac is an anhydrous salt, 5 grains of it ought to afford 8.2 cubic inches of gas; they actually afforded 7.5 cubic inches; and, in another instance, 1 grain which should have yielded 1.65 cubic inch. gave 1.56 cubic inch.

That Salammoniac is an anhydrous salt, even leaving out of consideration the results in the text, appears to be proved by the circumstance, that, when muriate of ammonia is formed by the direct union of the gases, and heat is applied to the salt, the smallest quantity of water present is driven off, and becomes visible, being condensed in the cool part of the vessel.—Vide, Nicolson's Journal, vol. xxxiv, p. 70. where I have described an experiment in proof of this.

data before given, in connexion with the analysis of Carrara marble and muriate of ammonia, that this salt is composed of

27.39 Ammonia,
54.58 Carbonic Acid,
18.03 Water; (considering the loss water)

or of

17. Ammonia (1 porportion 17)
34.2 Carbonic Acid (1½ 33)
10.8 Water (1 9)

an approximation, in regard to proportion, very close, excepting in that of the water, the excess of which, it may be conceived, is either hygrometrical, or, as is more likely, only apparent from inaccuracies committed with the experiments, and the data of the calculations.

The degree of solubility of the sesquicarbonate, and the effects of heat on it, have been variously stated by authors. I shall mention the results I have obtained in endeavouring to determine these points with some care, and also some other properties belonging to the salt, not unworthy of notice, in relation to the changes to which it is liable, depending on the operation of complicated affinities.

The quantity of sesquicarbonate which water is capable of dissolving, varies, I find, very much with the temperature:—Thus, 100 grains of water at 55° Fahr. were saturated with about 25 grains of the salt; at 62°, with about 30 grs.; at 90°, with about 37 grs.; at 105°, with about 41 grs.; and at 120°, with about 50 grs.*—the temperature at which the salt begins to be decomposed, as is indicated by the disengagement of gas.

It is commonly supposed, that a greater degree of heat than the temperature last mentioned, is required to decompose this salt; but incorrectly. This degree is as near the true one as possible. At 118° the solution remains tranquil; at 120°, air-bubbles are slowly disengaged; and above this, the effervescence rapidly increases in violence, with every additional degree of heat.

In mentioning the solubility of the sesquicarbonate at different temperatures, I have in every instance said about so much; for it is almost impossible to determine the quantity with absolute exactness, owing to the circumstance, that, when an excess of the sesquicarbonate is added, either a decomposition takes place in the saturated solution, or the salt added is decomposed, for a portion of neutral salt separates in the solid state, and the solution becomes more highly alkaline; and the same separation of neutral salt takes place on the reduction of temperature of a saturated solution of the sesquicarbonate, with a proportional increase of its alkalescence. Whether, in these instances, ammonia is evolved uncombined, or a carbonate of the same composition, as that formed by the direct combination of its element, I have not been able to ascertain.

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* The salt, in very fine powder, was added in *small* portions to the water in a bottle with a glass stopple, shaken well after each addition, and the temperature fixed by immersion in water.

The sesquicarbonate is said to be soluble in alcohol. I have not found it so. Thus, when 12.9 grs. of it, in fine powder, were added to 100 grs. of alcohol, of specific gravity .829, there was a slight reduction of temperature; after standing 24 hours, the bulk of salt was not apparently diminished; collected on a filter, and rapidly dried between the folds of bibulous paper, it weighed 11.8 grs. The alcohol had acquired a strong ammoniacal odour; mixed with dilute muriatic acid, only a very few minute air-bubbles were disengaged, no more than might be attributed to the separation of the little atmospheric air dissolved, which the diluted acid, and, I believe, the alcohol contain. The salt remaining on the filter was perfectly inodorous, and, in fact, was reduced to the neutral state. In this instance, it may be inferred that the sesquicarbonate was resolved into ammonia and the bicarbonate. The statical results are not inconsistent with this idea *; and the alcohol not effervescing, excepting in the slightest degree with an acid, confirms it. It is also confirmed by digesting alcohol, or a large quantity of the salt, so as to become strongly impregnated with ammonia. A solution of this kind, of specific gravity .834, did not, with an acid, give stronger indications of the presence of carbonic acid. When weaker alcohol is used, the result is somewhat different; judging from the degree of effervescence of the alcoholic solution, this formation of the neutral carbonate is accompanied by the pro-

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* No very rigorous calculation can be made on the result of an experiment, in which a very slight loss (hardly to be avoided) in filtration and drying would lead to considerable error.

duction of a little carbonate, as well as the separation of some ammonia; and the spiritus ammoniæ, and the spiritus ammoniæ aromaticus of the London Pharmacopœia, appear to be examples of this kind of solution; they effervesce distinctly with an acid, but less powerfully than a solution of the same strength of the carbonate of ammonia. *

3. *On the Bicarbonate of Ammonia.*

Of this salt I have examined various specimens, procured by four different methods: 1*st*, By the exposure of the sesquicarbonate to the atmosphere; 2*dly*, By passing carbonic acid gas, by means of Nooth's apparatus, into a strong solution of the sesquicarbonate; 3*dly*, By the action of alcohol on the sesquicarbonate, either in powder or in solution; and, 4*thly*, By heating sesquicarbonate in a saturated solution of this salt under pressure, and cooling the concentrated solution thus produced, also under pressure. The salt procured by the first and third method is in the state of powder; that obtained by the two other processes is crystallized, in one instance in rhomboidal plates; in the other, (by the second method), as is well known, in prisms, commonly four-sided, with pyramidal terminations.

The following table shows the results I have obtained, in subjecting specimens of this salt to the same process of analysis as that which was applied to the sesquicarbonate, and in the same quantities:—

Expt.		Carbonic Acid Gas.	Chloride of Silver.
1	-	1.18 cubic inch.	8.7 grains.
2	-	1.18	8.5
3	-	1.18	8.5
4	-	1.16	8.5
5	-	1.16	8.7
6	-	1.16	9.3
7	-	1.16	
8	-	1.16	8.3
9	-	1.16	8.4

The salt No. 1, 2, 3, was in rhomboidal crystals, formed in the manner described, and dried by exposure to the atmosphere, after having been washed with a little distilled water, to remove any adhering sesquicarbonate. The crystals were transparent and inodorous, and some of them were nearly half an inch in diameter.

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* From one experiment that I have made, alcohol of Sp. gr. .829, appeared to disolve about 50 times its volume of ammoniacal gas. The anhydrous carbonate of ammonia is also soluble in alcohol of this strength; but its degree of solubility I have not ascertained.

That No. 4 and 5 was in small prismatic crystals, and was also washed with distilled water, and dried in the atmosphere.

That No. 6 and 7 was in powder, procured by exposure of the sesquicarbonate for two or three weeks to the atmosphere, till it had lost its ammoniacal odour.

And that No. 8 and 9 was in powder, obtained by digesting the sesquicarbonate in alcohol.

Calculating on the maximum quantity of carbonic acid gas disengaged from one grain of the salt, viz. 1.18 cubic inch, and on the mean quantity obtained from five grains, namely, 8.6 grains, on the data before given, this salt would appear to be composed of

21.56	Ammonia,
56.01	Carbonic Acid,
22.43	Water;

or of

17	Ammonia, -	(1 proportion	17.)
44.1	Carbonic Acid -	(2 proportions	44.)
17.66	Water -	(2 ——	18.)

an approximation even greater than might be expected. In relation to the quantity of carbonic acid, it seemed preferable, in the instance of the sesquicarbonate, to take the maximum for forming the estimate; and in that of the bicarbonate the maximum, as the tendency of the former salt is to pass into the neutral; and of the latter salt to retain a little of the sesquicarbonate, and probably to generate it.

Considering the methods of analysis employed, I am disposed to think that the slight differences in the results, in the several instances, arise, not so much from imperfection in the experiments, as from slight difference of composition in the salts themselves, which, if I do not deceive myself, will readily be admitted, after the statement of some particulars, which I shall now proceed to give concerning the bicarbonate.

This salt is less soluble in water than the sesquicarbonate, and more readily deprived of a portion of its carbonic acid, by heat, and other disturbing causes. From the trials which I have made of its solubility, water appears to take up about 17 per cent. of it, at 55°. When I have added more salt, even at this temperature, a portion has disappeared, but not without the disengagement of air-bubbles; and, at a higher temperature, as at 98°; and above that, the disengagement of gas has been copious, indicating a decomposition of the salt, which is also denoted by its acquiring an ammoniacal odour. So great is the tendency of the bicarbonate to undergo change with water, that, though I have just spoken of its solution, I am doubtful if a true solution of it can be obtained, or if it can exist in the state of aqueous solution. I have been led to entertain this doubt, by finding it impossible to saturate a solution of the sesquicarbonate, by passing carbonic acid gas into it in Nooth's apparatus: however great was the quantity of carbonic acid gas introduced, the solution continued alkaline; none of the acid appeared to be arrested, excepting that portion which was fixed in the bicarbonate, the bicarbonate itself becoming solid and crystalline as it formed. And I have been strengthened in the doubt, by my inability to preserve either a weak or a strong solution, made with the neutral salt, for any time, without its becoming alkalescent; even immediately after the solution of inodorous crystals, test-paper has indicated an excess of alkali. Even on the solid salt, the presence of moisture acts as a disturbing cause. Crystals, when put by, though perfectly inodorous, and apparently dry at the moment, soon acquire, in close vessels, a distinct and even strong ammoniacal odour, accompanied by the separation of moisture,—part of the water of crystallization; and this, I have observed, even takes place immediately, after removal out of an atmosphere of carbonic acid gas, where the salt had been exposed to considerable pressure. How to explain the change seems difficult; at least, keeping in view the formation of the salt, it seems, as it were, a kind of link between inorganic and organic chemistry. The influence of water, however, appears to be certain, and the only way in which I have been able to preserve the bicarbonate for any length of time, has been by keeping it perfectly dry, and putting into the bottle with it chloride of calcium wrapped in paper.

The bicarbonate exposed to the air, seems to undergo the same decomposition as when it is confined under ordinary circumstances; and in time it entirely disappears, and the more rapidly, the greater the humidity of the air. If a strong solution is exposed, the decomposition is very rapid; it presently emits an ammoniacal odour, which it continues to give out till all the saline matter is dissipated, and only water remains. The method of obtaining the bicarbonate by the exposure of the sesquicarbonate to the atmosphere, may appear incongruous with this statement; but I do not consider it so in reality, at is might not be difficult to show, were the subject sufficiently interesting to entitle it to minute discussion.

The other methods mentioned, of procuring the bicarbonate, might be adduced in favour of the idea, that it can only exist in the solid state, particularly its production in a strong solution of the sesquicarbonate in cooling, and the more especially, as it takes place, even if part of the carbonic acid gas is allowed to escape from the heated solution, or if aqua ammoniæ is added to the solution.

The bicarbonate, I believe, is perfectly insoluble in alcohol, and its production from the sesquicarbonate, by the action of alcohol, may be partly owing to this circumstance. When kept in alcohol, it undergoes change as in air, carbonic acid gas is disengaged, and a solution of carbonate of ammonia is generated.

4. *On the effect of Heat on the solid Sesquicarbonate, and Carbonate of Ammonia*

It was in an experiment on subjecting the sesquicarbonate of ammonia to heat, as already observed, that my brother, in 1799, inferred that he had obtained a new combination of carbonic acid and ammonia, characterized by its acrid ammoniacal odour and deliquescence in the atmosphere; and containing, as he believed, a larger portion of ammonia than any of the then known compounds.

As far as mere observation is concerned, the results I have obtained in repeating the experiment are in perfect accordance with his. The products of the distillation of the sesquicarbonate appear in the following order:—First, carbonic acid gas; next, the salt in question, which, if applied, is considerable, and no cooling means employed, reaches the neck of the retort, and even passes into the receiver: next, a salt which appears to be a mixture of the preceding, and of the sesquicarbonate; and lastly, the same mixture with a great excess of water.

Of these saline products, none that I have examined by the methods of analysis described, have contained more ammonia than the carbonate. The salt which is most volatile, and consequently passes farthest before it condenses, contains about the same proportion of ammonia as the anhydrous carbonate, and appears to differ from it only in containing water,—in fact, in being a hydrated carbonate. Dr Thomson, in the last edition of his System of Chemistry, states, that "the carbonate may be formed by mixing one volume of carbonic acid gas, two volumes of ammoniacal, and one volume of the vapour of water *." I have thus obtained it over mercury; and it has exhibited the same properties as that which my brother first procured by distillation, especially the property of deliquescing on exposure to the atmosphere, of which no mention is made by Dr Thomson, or by any other chemical writer that I have had it in my power to consult.

Relative to the statement of my brother, contained in his researches already referred to, that the proportion of alkali capable of combining with carbonic acid may vary between 20 and 50 per cent., according to the temperature at which the combination is made; I believe he was deceived by the methods of analysis which he employed, as none of the trials which I have made have confirmed his conclusion. I shall mention the results of one experiment only, in which the carbonate of ammonia carefully made with dry gases, over dry mercury, was exposed to a temperature of about 170°, gradually raised, through the medium of a mercurial bath. As long as the temperature was below 140°, the salt appeared unaltered, no gas was disengaged; proving that there was no change of composition. When the temperature was raised to 140°, the salt presently became gaseous, and, as well as I could judge, occupied just the same space as its ingredients did before combination. When reduced below 140°, the gaseous salt, whether in the upper part of the jar, or

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* System of Chemistry, ii. p. 384,—7th edition.

transferred into another jar, (which was very easy to do, by protecting the hands with thick leather gloves), was again condensed, without any gaseous residue, and this equally, whether previously heated to 140°, 160°, or 170.°

MALTA, *May 8th*, 1833.

STATISTIC VIEWS OF THE MORTALITY IN VARIOUS COUNTRIES IN EUROPE.

IN considering how limited and few the differences are which exist among the nations of Europe, either in their physical or moral order, it appears at first sight that the laws to which the duration of human life are subjected, cannot experience any very great variations even in the most distant countries. It would be erroneous, however, to believe that it is so; for in the group of European nations who live under the same zone, and whose primitive features are gradually effaced by civilization, mortality experiences as long a series of different terms as in regions inhabited by different races of men, and situated in climates the most opposite to each other.

The causes which affect the movements of the population in Europe, have even a more powerful influence on mortality than on reproduction or births. The greatest is nearly twice as great as that of the least fecundity; whilst, in many districts, the annual amount of deaths is generally three times as much as that to which it is limited in some others in proportion to the whole population.

In truth, the most accurate statistical returns which have been drawn up prove, that mortality is reduced in various countries to such a point, that it does not exceed one death in 59 inhabitants; whereas in many others, it increases annually to one twenty-second part of the population, and still more immensely in times of distress.

On searching in official documents for the number of deaths for several of the latter years, in the principal European states, it is found, that the difference of their mortality compared with their population is as follows:

Table of the proportion of deaths to the population, and of the annual mortality among each million of inhabitants in the principal States of Europe.

Countries.	Periods or Epochs.	Average number of Deaths.	Ratio of the average Population.	Annual mortality in each million of inhabitants.
Sweden and Norway,	1821 to 1825	79,900	1 in 47 inh.	21,300
Denmark,	1819	33,800	1 ... 45 ...	22,400
European Russia,	1826	960,000	1 ... 44 ...	22,700
Kingdom of Poland,	1829	93,000	1 ... 44 ...	22,700
Britannic Islands,	1818-1821	373,000	1 ... 55 ...	18,200
Netherlands,	1827-1828	163,000	1 ... 38 ...	26,500
Germany Proper,	1825-1828	290,000	1 ... 45 ...	22,400
Prussia,	1821-1826	303,500	1 ... 39 ...	25,600
Austrian Empire,	1828	675,000	1 ... 40 ...	25,000
France,	1825-1827	808,200	1 ... 39 ...	25,600
Switzerland,	1827-1828	50,000	1 ... 40 ...	25,000
Portugal,	1815-1819	92,000	1 ... 40 ...	25,000
Spain,	1801-1826	307,000	1 ... 40 ...	25,000
Italy,	1822-1828	660,000	1 ... 30 ...	33,300
Greece,	1828	33,000	1 ... 30 ...	33,300
Turkey in Europe,	1828	334,800	1 ... 30 ...	33,000
Northern Europe,	. . .	2,972,100	1 ... 44 ...	22,700
Southern ditto,	. . .	2,284,200	1 ... 36 ...	27,800
TOTAL,	. . .	5,256,300	1 ... 40 ...	25,900

According to this table, and many others of a more detailed nature, there annually dies:

1 inhabitant in 28 in the Roman State, and the ancient Venetian provinces; 1 in 30 in Italy in general, Greece, and Turkey; 1 in 39 in the Netherlands, France, and Prussia; 1 in 40 in Switzerland, the Austrian Empire, Portugal, and Spain; 1 in 44 in European Russia and Poland; 1 in 45 in Germany, Denmark, and Sweden; 1 in 48 in Norway; 1 in 53 in Iceland; 1 in 58 in England; 1 in 59 in Scotland and Iceland.

These numbers present the following results:

The smallest chances of life and its shortest duration are not, as one might believe, amongst the effects of the rigorous climate of Norway, or the marshy soil of Ireland; it is in the beautiful climate of Italy that life is reduced to its minimum extent. On the contrary, it is among the icy rocks of Iceland, in the midst of the eternal fogs of Scotland, where man attains his greatest age.

Of all the European States, the British isles are, in this respect, the most favoured; of each million of inhabitants, they only lose 18,200, whilst the mortality is almost double in the countries washed by the Mediterranean.

Life, next to these, is most certain in Norway and Sweden. *Cæteris paribus*, three people die in the south of Europe, two only and hardly that, die in ancient Scandinavia. Denmark and Germany enjoy, as it were, similar advantages.

Russia and Poland, where nature and fortune have not been lavish of the necessaries of life, enjoy, however, an astonishing longevity. The population, comprising a mass of nearly sixty millions, spin out their existence almost one half longer than that which is enjoyed by the inhabitants of Italy, and exactly twice the length of time which any one can expect to live in the capital of Austria.

The average time of life which only cuts off one victim in 40 annually, is to be found in the Cantons of Switzerland, the Austrian Provinces, and in the Spanish Peninsula, in consequence of the sun and the dryness of the air.

France, the Netherlands, and Prussia, nearly reach the same term, and will soon get beyond it, unless war or some other scourge arrests the progress of their social amelioration.

In the rest of Europe, the mortality amounts to one-thirtieth of the population, and is even frequently increased by accidental causes, which have for a long time endangered the prosperity of the shores of the Mediterranean.

On the whole, we reckon, one year with another, 5,256,000 deaths in 210 millions of people, by a mortality of one fortieth part, which is unequally distributed among the northern and southern states. The former have only one death in 44, the latter, one in 36 persons. Of one million of inhabitants in districts situated in the north of France, 22,700 die; 27,000 die in those which lie towards the south. This is a difference of more than 5000 deaths, equivalent to a two-hundredth part of the population.

If we carefully examine these numbers and those of the tables of details from which they are extracted, we will discover that two great causes which predominate over all the others, determine the ratio of mortality to the population, or, in other words, regulate the number of the chances of human life. These are the influence of climate and civilization.

The climate is peculiarly favourable to the prolongation of human life when it is cold or even rigorous, or when the humidity in the environs of the sea is combined with a low temperature.

The smallest mortality in Europe, occurs in maritime countries which are in the vicinity of the polar circle, such as Sweden, Norway, and Iceland. This is also the case in countries where, as in Russia, the influence of climate is not aided by civilization, and of itself is sufficient to assure long life to mankind.

Countries where the heat is moderate, are not, as might be supposed, among those which possess the advantage of a small mortality; to obtain it, they must acquire the benefits of a high state of social order.

The southern countries, the mild climate of which seems to be so favourable to the human race, are, on the contrary, regions where life is exposed to the greatest dangers. In the smiling plains of Italy, the chances of dying are one-half greater than those of cold and foggy Scotland; and under the beautiful skies of Greece, the certainty of life is one-half less than among the ices of Iceland.

The places of the torrid zone, the mortality of which has been calculated, show the pernicious influence which is exercised over the existence of mankind by a high temperature.

Latitude.					
6° 10′	Batavia,	decrease of	1 in 26½	inhabitants.	Burrow.
10° 10′	Trinidad,	——	1 — 27	——	Official documents.
13° 54′	St Lucie,	——	1 — 27	——	Pugnet.
14° 44′	Martinique,	——	1 — 28	——	M. de J.
15° 59′	Guadaloupe,	——	1 — 27	——	M. de J.
18° 56′	Bombay,	——	1 — 20	——	Transactions Acad.
23° 11′	Havannah,	——	1 — 33	——	Humboldt.

The resistance of the vital principle in the tropics differs according to the races of men; and its duration in some places is, for the one, double or triple that for the others. The following are examples of this variation:—

Batavia, in 1805—Europeans, 1 in 11 individuals; Slaves, 1 in 13; Chinese, 1 in 29; Javanese, 1 in 46. Bombay, in 1815—Europeans, 1 in 18¼; Mussulmen, 1 in 17½: Parsees, 1 in 24. Guadaloupe, 1816 to 1824—Whites, 1 in 23½; Freedmen, 1 in 35. Martinique, in 1815—Whites, 1 in 24; Freedmen, 1 in 33. Grenada, 1811—Slaves, 1 in 22.—St Lucia in 1802—Slaves 1 in 20.

We may compare this immense mortality in the torrid zone, with that which occurs in Madeira, the only colonial establishment in the temperate zone. Heberden has calculated, that the deaths in that island were in the proportion of 1 to 49.89, of the whole inhabitants.

The effects that the degree of perfection, more or less extensive, of the social economy exerts on mortality, are not less extensive than those the cause of which is to be found in the influence of the climate.

The influence of the progress of civilization is discovered by comparing the ratio of the deaths to the population of the same country at epochs, the intervals of which have been marked by social ameliorations. The following series of universal terms presents an instructive comparison :—

The number of deaths compared with that of the inhabitants, was,

In Sweden,	1754 to 1763,	1 in 34,	in 1821 to 1825	1 in 45
— Denmark,	1751 to 1756	1 — 32	—1819	1 — 45
— Germany,	1788	1 — 32	—1825	1 — 45
— Prussia,	1717	1 — 30	—1821 to 1826	1 — 39
— Wurtemberg,	1749 to 1754	1 — 32	—1825	1 — 45
— Austria,	1822	1 — 40	—1828 to 1830	1 — 43
— Holland,	1800	1 — 26	—1824	1 — 40
— England,	1690	1 — 33	—1821	1 — 58
— Great Britain,	1785 to 1789	1 — 43	—1800 to 1804	1 — 47
— France,	1776	1 — 25½	—1825 to 1827	1 — 39½
— Canton of Vaud,	1756 to 1766	1 — 35	—1824	1 — 47
— Lombardy,	1769 to 1774	1 — 27½	—1827 to 1828	1 — 31
— Roman States,	1767	1 — 24	—1829	1 — 28
— Scotland,	1801	1 — 44	—1821	1 — 59

Thus the mortality has diminished,—

In Sweden, nearly ⅓ in 61 years; in Denmark, ⅖ in 66 years; in Germany ⅖ in 37 years; in Prussia, ⅓ in 106 years; in Wurtemberg, ⅖ in 73 years; in Austria 1/13 in 7 years; in Holland ½ in 24 years; in England, ⅘ in 131 years; in Great Britain, 1/11 in 16 years; in France, ½ in 50 years; in Canton of Vaud, ⅓ in 64 years; in Lombardy, ⅐ in 56 years; Roman States, ⅓ in 62 years.

For thirty years, the mortality has been stationary in Russia and Norway; it has increased in the kingdom of Naples.

On the whole, there has been, for 80 years, a mortality of 1 individual in 36 throughout Europe, according to Süssmilch; our calculations do not exceed 1 in 40, according to the average of later years. On the continent of Europe there has, therefore, been a diminution of one-ninth in the mortality of the aggregate of the people, if we can rely on the German writer on statistics. We are, however, inclined to think, that, during his time, the mortality generally was less than one-thirtieth of the population, which supposes that it is more than one-third less at present in proportion to the augmentation of its population.

This gradual diminution of mortality arises from the same causes in the principal towns of Europe. The number of deaths compared with that of the inhabitants at distant epochs, gives the following proportions:—

Paris,	in 1650	1 in 25 inhab.;	in 1829	1 in 32
London,	... 1690	1 ... 24 ...	... 1828	1 ... 55
Berlin,	... 1755	1 ... 28 ...	... 1827	1 ... 34
Geneva,	... 1560	1 ... 18 ...	... 1821	1 ... 43
Vienna,	... 1750	1 ... 20 ...	... 1829	1 ... 25
Rome,	in 1762–1771	1 ... 21 ...	... 1828	1 ... 31
Amsterdam,	in 1761–1770	1 ... 25 ...	... 1828	1 ... 29
Cambridge,	in 1811	1 ... 41 ...	... 1821	1 ... 58
Norfolk,	... 1811	1 ... 50 ...	... 1821	1 ... 59
Manchester,	... 1757	1 ... 25 ...	... 1821	1 ... 58
Birmingham,	... 1811	1 ... 30½ ...	... 1821	1 ... 43
Liverpool,	... 1773	1 ... 27 ...	... 1821	1 ... 41
Portsmouth,	... 1800	1 ... 28 ...	... 1811	1 ... 38
Petersburg,	... 1768	1 ... 28 ...	... 1828	1 ... 48
Stockholm,	in 1758–1763	1 ... 19 ...	... 1827	1 ... 26

The annual mortality has also diminished in Paris, more than ⅕ in 80 years; in London, more than ½ in 178 years; in Berlin, ⅕, or nearly ¼, in 72 years; in Geneva, ⅗ in 261 years; in Vienna, ¼ in 80 years; in Rome, ⅓ in 63 years; in Amsterdam, ⅙ in 64 years; in Cambridge, ⅖ in 10 years; in Norfolk, ⅙ in 10 years; in Manchester, ⅗ in 64 years; in Birmingham, nearly ⅖ in 10 years; in Liverpool, ⅓ in 38 years; in Portsmouth, more than ⅕ in 11 years; in Petersburgh, nearly ⅔ in 40 years; in Stockholm, more than ⅓ in 67 years.

The causes of the greatest mortality in European countries and cities are chiefly—

The marshy humidity of the air, especially in hot countries; the effects of privations on the lower classes of society; the scarcity of the means of subsistence, or, at least, their rise in price as compared with the wages of labour; pestilential diseases; unfavourable seasons, especially abrupt changes in the temperature; the closeness, dirtiness, and unhealthiness of private houses, prisons, infirmaries, and hospitals; the excessive use of spirituous liquors, and indulgence in drunkenness; unwholesome or unremitting labour, especially in childhood and youth; lastly, war, but less in consequence of battles than forced marches, and frequently the mal-administration of armies.

The causes of the diminution of mortality where civilization is progressive, are—

The draining of marshes, and the embanking of streams and rivers; the favourable division of public wealth, which affords each individual labour and subsistence; the abundance and good quality of the food of the people; the attention bestowed on children from birth, and continued in schools, manufactories, and public establishments; vaccination, and sanitary arrangements, which prevent the importation or development of contagious diseases; the low price of the productions of industry, allowing the less affluent classes to enjoy those habits of cleanliness which were equally unknown and beyond their reach, and furnishing them with the means of escaping the intemperateness of the season; lastly, the successful measures adopted for diminishing the insalubrity of towns, and especially of colleges, theatres, hospitals, prisons, churches, and other public establishments, which, in many places, are still without the means of ventilation, heating, and cleaning.

The results of such ameliorations may be appreciated in a convincing manner, by inquiring what has been their influence on mortality during the last century in the three European countries where their progress has been most obvious. If we collect England, Germany, and France in one group, we find, that the average term of mortality which, in that great and populous region, was formerly 1 in 30 people annually, is not, at present, more than 1 in 38. This difference reduces the number of deaths throughout these countries from 1,900,000, to less than 1,200,000 persons; and 700,000 lives, or 1 in 83 annually, owe their preservation to the social ameliorations effected in the three countries of western Europe, whose efforts to obtain this object have been attended with the greatest success.

The life of man is thus not only embellished in its course by the advancement of civilization, but is even extended by it, and rendered less doubtful. The effects of the amelioration of the social condition are to restrain and diminish, in proportion to the population, the annual number of births, and in a still greater degree that of deaths; on the contrary, a great number of births, equalled or even exceeded by that of deaths, is a characteristic sign of a state of barbarism. In the former case, as men in a mass reach the plenitude of their physical and social development, the population is strong, intelligent, and manly; whilst it remains in perpetual infancy where generations are swept off without being able to profit by the past, to bring social economy to perfection.

ON ELECTRICITY, AND ON THE NATURAL SOURCES OF ELECTRICITY AND MAGNETISM. *By Professor M. A. De la Rive.*

I. *Atmospheric and Animal Electricity.*

Concerning atmospheric electricity, the beautiful work in the Memoires de l'Academie des Sciences, upon lightning conductors, ought not to be overlooked. This memoir *, drawn up by M. Gay Lussac, is composed of two distinct parts. In the former, he furnishes a very lucid and complete summary of the theory of thunder and conductors. He briefly recapitulates the electrical phenomena upon which this theory is founded, and the results which observation and experience have afforded on the subject. He dwells upon the dangers of imperfect conductors, and of such as are interrupted, or not sufficiently large to resist the heating influence of the lightning. He also insists upon the necessity of their having as free an entrance as possible into the soil beneath. In accordance with the opinion of M. Charles, who attended a great deal to this subject, he believes that a conductor will thoroughly protect from the assaults of lightning, a circular space having a radius of a length double the height of the conductor; a rule, according to which buildings ought to be protected. He finally cites a great many instances in which accidents have occurred from inattention to these precautions, or from a total want of a conductor. He alludes to a remarkable fact, which seems to prove that movement of the air facilitates the descent of the thunderbolt, viz., that, in 1718, in the night between the 14th and 15th of April, four-and-twenty church towers, in all of which the bells were rung, were struck with lightning in Brittany; whilst those escaped in which the bells were not rung. He protests then strongly against the prevailing practice, especially in villages, of ringing the bells on the approach of a storm, with the purpose, it is alleged, of dissipating and breaking up the thunder cloud.

The practical part of the memoir includes a great number of interesting details upon the construction of conductors. It is in particular recommended, that the vertical rod shall be 28 or 30 feet high, of a pyramidal form, with a base of 2 inches on each side, and, to prevent the oxidation of the iron, that it should terminate in a point of copper, gilt, or platina. The conductors ought to be either iron-bars of seven or eight square lines, or iron-wire of the same diameter. These conductors should be solidly fixed, by a metallic joining, to the vertical rod, and this again should descend into a well, if there be one in the neighbourhood, so as to plunge 2 feet at least into the water; and if there be no wells, then they ought to be buried in a hole 10 or 15 feet deep, and surrounded with coal-coom, which has the double advantage of facilitating the flow of the electric fluid, and of preserving the iron from rust. When the edifice is large, and it is deemed necessary to have more than one conductor, it is advantageous they should all communicate with one another by means of iron-bars, thus uniting them into a complete whole. There is, moreover, in the memoir, many valuable hints concerning the construction of the several pieces of conducting apparatus, and also of the most eligible method of arranging them, answering to the plan of the building to be protected. Thus it is said, that, in the case of steeples surmounted by a cross, it is sufficient to establish a communication betwixt the cross and the earth, by a good conductor, to preserve both the steeple and the neighbouring building. We may here add, though there is nothing said of it in the memoir, that experience has proved, that those mansions the roof of which are tolerably covered with white iron, communicating with the earth by means of metallic water-pipes, are, by these means, well sheltered from the lightning, and may be considered as protected.

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* Vide Ann. de Chim. et de Phys. t. xxvi. p. 258.

We would here add a word on the electricity developed by the Torpedo. Since Sir H. Davy's investigations on the subject, unequivocal signs of electrical currents have been obtained by touching certain points of the body of the animals with the two extremities of the galvanometer. At the recommendation of the illustrious chemist, these researches were undertaken by his brother Dr Davy, who, whilst in Malta, could easily procure the living animals. By applying the extremities of the metallic wire of the galvanometer to the two extremities of the electrical organ, there was established in the wire a current capable not only of affecting the needle, but also of strongly magnetizing needles that before were not magnetized; this current could also decompose a saline solution, and produce shocks; but the author could not succeed in discovering sensible traces of heat or of light.

The direction of the current indicated, that it was the inferior surface of the electrical organ that was the source of the negative electricity, and the superior of the positive. Respecting the anatomical structure of this organ, Dr Davy has furnished a detailed description, whence he concludes, that it is not muscular, but that the columns of which it is composed are formed of tendinous or nervous fibres, filled with a gelatinous fluid, and a great quantity of water, which may be disengaged by evaporation.

II. *On Terrestrial Magnetism, as connected with the Origin and Formation of the Earth.*

Besides atmospheric and animal electricity, there is a third natural source, on which it may be interesting to give a few details. We allude to that better known under the name of terrestrial magnetism, and which consists in the probable existence of electrical currents under and very near to the surface of our globe. In former times, terrestrial magnetism was supposed to be connected with two magnetic poles, which received a local habitation at no great distance from the geographic ones; but since we have succeeded in demonstrating the identity of magnetism and electricity, and, more especially, since we have seen that electrical currents can impart to the magnetic-needle a determinate direction, the much more probable supposition of terrestrial electrical currents has been embraced, to explain the directive action of the globe upon the magnetic-needle. The extensive and successful study of terrestrial magnetism, seems more favourable to this hypothesis than to any other; and it seems to accord in the most remarkable manner with the constitution of the globe, and with the phenomena that are taking place beneath and upon its surface. Some slight details will, we trust, demonstrate the accuracy of this assertion.

There are two circumstances which, having been carefully observed in many different places of the earth's surface, have elucidated the theory of terrestrial magnetism, viz. the *direction* of the attraction exercised upon the needle by the earth, and the *intensity* of that attraction. On the former of these circumstances, as is well known, hangs two results, first the *inclination* or dip, the angle at each spot formed by the needle, and the perpendicular; and secondly, the *declination*, or the angle which this same direction forms with the astronomical meridian, or true north and south. Since the publication of M. de Humboldt's observations about the beginning of the present century, a great number of travellers and naturalists have made and published observations of a similar kind. Of these reseaches, so assiduously prosecuted, or still going forward, we can cite only a very few of the most remarkable.

The numerous voyages of discovery which have been undertaken within the last few years, and especially towards the North Pole, have enabled M. Hansteen, from the observations of navigators, and from those which he himself has made, especially in Siberia, to draw ingenious conclusions concerning the position of the magnetic poles, and the *isodynamic lines*, that is, those lines which pass through all those points where the magnetic intensity is the same. The arrangement of these lines, which, for a long time, was imagined to be that of small circles nearly parallel to the magnetic equator, is not so regular; on the contrary, it seems to indicate the existence, in each hemisphere, of two distinct magnetic poles, each of which forms a centre, around which the isodynamic curves are distributed sufficiently regularly. These poles are situated, so far as the northern hemisphere is concerned, the one in Hudson's Bay, and the other in Siberia: the latter of these appears to be somewhat more feeble than the former.

The observations of Captains Sabine and Freycinet, and especially those of Captain Duperrey have supplied a great number of data, as valuable for their great accuracy, as for the choice localities in which they have been made. Those of Captain Duperrey, taken in the voyage of discovery which he made in *La Coquille*, during the years 1822–23–24 and 1825, have enabled us to determine the real position of the magnetic equator, viz. of the curve which passes through all the points where the needle exhibits no inclination or dip, but remains perfectly horizontal. It is known that the general direction of this curve, is that of a great circle of the terrestrial sphere, forming an angle of some degrees with the terrestrial equator. This was known as early as 1780, and more accurately determined by M. Morlet, in consequence of the observations of M. de Humboldt. The observations of Captain Duperrey shew, that it is liable to a movement of change, which, from year to year, progressively transports it from east to west. The observations so accurately made in 1829, on board the *Uranie* by Captain Freycinet, indicated a displacement in the point of intersection of the magnetic and terrestrial equators; and it follows from those of Captain Sabine, that this point is now about 18° more to the west than it was in 1780. M. Kupffer, at the close of his researches on different points of terrestrial magnetism, has recognized another movement from east to west in the line without declination, that is, in the line which runs through all the successive points where the needle points straight north. It is evident that these two motions are intimately allied, and that they give an explanation of the fact, that in each place, from year to year, the declination, and the inclination, increase or diminish, in a regular manner. We wish we could, in addition, cite those observations of M. Kupffer, which relate to the variations in the mean duration of the oscillations of the needle in the same place, variations which seem to depend rather on those of the magnetic inclination, than on those of its intensity. M. Arago, who for a great number of years has been making numerous important inquiries on this subject, has moreover clearly detected diurnal variations in the inclination, and even in the intensity of terrestrial magnetism.

Mr Dove has recently published, along with a preface by M. de Humboldt, corresponding observations on the regular hourly variations, and the irregularities of the magnetic dip, in the central and eastern parts of Europe. From these labours it results, that in each point the march of the magnetic oscillations, like that of the oscillations of the barometer, appear to depend on the apparent diurnal movements of the sun; that the extent of the whole deviation is greater in summer than in winter; and that the time of the maximum of the western deviation falls, during the whole year, between one and two o'clock in the afternoon. Respecting the remarkable perturbations of the needle which M. de Humboldt had observed, and which he has denominated *magnetic storms*, Mr Dove has cited instances which prove that they coincide, as M. Arago had also proved, with the phenomena of the Aurora-borealis, even in countries far distant from places where the Aurora-borealis is visible. This coincidence is moreover supported by very many additional observations; such, for example, as Mr Fox reports, in his researches on the variations of the intensity of terrestrial magnetism. But the most of them are collected in the Annales de Chimie et de Physique, t. xxxix., where they have been regularly published by M. Arago, who has himself engaged in an interesting discussion on the subject, in answer to some objections which had been made to his views by Dr Brewster.

Though obliged to pass by many of the researches on the phenomena of terrestrial magnetism, we wish still to mention those which some American naturalists have undertaken on the declination of the needle in the United States; and also the observations of M. Quetelet upon the measure of the magnetic intensity in different places in Germany, the Netherlands, of Switzerland and Italy. The former lead to consequences altogether similar to those which Europeans, and especially M. Hansteen, have drawn from their observations. The latter, those of M. Quetelet, executed with unusual care, afford the horizontal intensity of terrestrial magnetism, for a great number of places in the countries named above; and as it regards the total intensity, it can be determined only for those of the places where the magnetic dip is known. The horizontal intensity goes on increasing as we approach the south, a natural consequence of the diminution of the dip; the total intensity, on the contrary, in spite of one or two anomalies, seems to increase as we advance northwards. We take leave, on the point of these magnetic intensities, to allude to the remarkable coincidence which M. Necker de Saussure has found to exist between the direction of curves of equal magnetic intensity, and that of the stratification of the principal chains of mountains, and of the physiognomy of continents*. This relation which M. Necker has succeeded in establishing with much precision, supporting it upon the numerous magnetic observations with which science has been enriched for several years, and especially those of M. Hansteen, is assuredly not a circumstance purely fortuitous, and presents much to interest in a geological as well as in a philosophical point of view.

The examination which we have just made of the results to which observations on terrestrial magnetism have hitherto led, however rapid it may have been, is nevertheless sufficient to enable us to judge of the theories by which it has been attempted to explain this remarkable phenomenon of our globe. The anomalies which the direction and the intensity of magnetic forces present in some places, and the changes which, in all, they invariably undergo along with time, cannot be reconciled with the theory of magnetic poles; unless we consent to multiply their number, and to attribute to them a movement which would make them to be always changing their site; hypotheses too improbable, and too much opposed to the known phenomena, to be admitted. It may easily be perceived that the supposition of electrical currents, distributed over all the surface of the globe, at the same time that it satisfactorily explains the general direction of the needle, can much better than any other account for the anomalies and irregularities to which in some places this direction is liable: all indeed that it is necessary to admit is, that it is not improbable that some particular circumstance may slightly modify the direction and intensity of the electrical currents in the places where they occur.

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* Bibl. Univ. t. xliii. p. 166.

But whence can these terrestrial currents originate? And how can their existence agree with the constitution of the globe, and the different phenomena which it presents? These are the questions to which we would now give an answer, by supplying a very abridged exposition of the ideas that have been elicited on the point. Before, however, commencing this subject, which will close this communication, we take leave now to oppose an hypothesis by which it has been proposed to explain terrestrial currents, as conceiving them owing to the simple rotatory motion which the earth experiences. This theory, which is based on the action which Arago has discovered to be exercised upon the needle by a revolving disc, cannot be applied to the case of the terrestrial globe, because the needle participates in its movement, and by consequence is, to it, in a state of relative rest: On the other hand, the necessary condition for the development of magnetism by rotation, is the relative movement of the needle, and the body which is to act upon it.

It is necessary then to seek elsewhere than in the needle itself, for the origin of terrestrial currents. Starting with the idea first thrown out by Davy, that the globe is formed of a metallic nucleus, covered over with an oxidized crust, M. Ampère thought that he found the source of these currents in the chemical action which the unoxidized surface of the nucleus must exert upon the water, and other agents which may come into contact with it. This hypothesis, founded on the nature of earthy substances, which are only metallic oxides, seems also to give a satisfactory explanation of all the other phenomena of the physical earth. It has nevertheless been attacked, and another has been proposed to be substituted, which consists in the supposition, that the globe has been originally, in consequence of the high temperature, in a state of complete fluidity; that it has then cooled down by degrees; that this cooling has produced the solidification of the exterior crust, which, always going on, produces an increase in the thickness of the shell, which notwithstanding is very feeble in comparison to the extent of the central liquid mass. This theory derives support principally from the spheroid form of the earth, which could only result as the effect of rotation on a liquid mass; also from the regular increase of temperature which is constantly observed so soon as we penetrate a little below the surface of the soil; from the appearance of volcanoes, the products of which arise from the burning materials shut up in the interior of the globe; from earthquakes, which are the effects of efforts made by the materials, upborne by the elastic force of gases and vapours, to escape;—these are a few of the phenomena, amongst many others, which are invoked as arguments in favour of the opinion to which we have been just adverting.

To explain the terrestrial currents, recourse must be had, in this theory, to the calorific action of the solar rays: it may be supposed that the surface of the earth is covered with thermo-electrical currents; which are produced by the differences of temperature, which exist between those parts of the surface which have been heated by the sun, and those which have not yet been heated. But this difference of temperature is so feeble; the substances which form the surface of the earth are such bad conductors of heat and electricity, and the heat itself is so irregularly distributed, that it is difficult to find, in this cause, a source of the currents sufficiently intense, and disposed in a manner sufficiently regular, to explain the phenomena of terrestrial magnetism. Probably we may, with more likelihood, attribute to these thermo-electrical currents, developed by the calorific action of the sun upon the surface of the earth, the diurnal variations, corresponding to the apparent motion of this luminary, which the direction of the magnetic needle, and the intensity of the forces which act upon it, experience. The extent of these variations, in our latitudes greater in summer than winter, and in the hottest period of the day than in the night, seems very favourable to this opinion. But we must not go too far, nor wish to push beyond the limits which propriety warrants this influence of thermo-electrical currents.

We are thus forced to return to the theory of M. Ampère for an explanation of the origin of electrical terrestrial currents, and consequently to that of Davy respecting the constitution of the globe. And it may now be enquired, whether this last opinion be of such a nature, that it is impossible to explain without it the spheroidal shape of the globe, and the several phenomena that are occurring, whether on the surface or beneath it? This is the inquiry upon which M. Ampère, in his lectures, has thrown out some ideas pre-eminently ingenious, and which M. Roulin has communicated in the *Revue des Deux Mondes*, tom. iii. p. 96.*

After remarking, that the fact upon which the partisans of the liquidity of the interior of the earth principally rests, viz. the increase of temperature as we penetrate beneath the surface, is not at all contrary to the supposition of the existence of a solid nucleus, exposed at its surface to chemical action, and thus a source of abundant caloric, we cannot but observe that it is almost folly to infer from what occurs in the $\frac{1}{1400}$th part of the diameter (for no one has ever penetrated a league), what is passing throughout the whole of the extent. On the contrary, it is an unalterable law in physics, that we ought not to consider a law as general, unless it has been clearly observed in the greatest part of the scale; and assuredly this rule is disregarded, when we deduce, from observations made within such narrow limits, the conclusion that the temperature goes on increasing gradually to the centre, or at least to this liquid mass. Those who admit this interior fluidity of the earth, seem, moreover, not to have thought on the influence which the moon would exert upon this enormous liquid mass; an influence which would produce tides, analogous to those of our seas, but far more terrible, both from their size, and the great density of the liquid. It is difficult to conceive how the earthy crust could resist this power, if, as must be granted in this hypothesis, a void space exists between the solid covering and the liquid mass.

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* Revue des Deux Mondes, tom. iii. Second Series (1r. Juillet 1833), p. 96.

That we may thoroughly analyse, and be able to prove the theory of a solid nucleus, we must go back to the first formation of the earth. This is what M. Ampère has done. He begins by admitting, as Herschel had proposed, supporting his views on the appearance of celestial bodies generally, and more especially of the Nebulæ, that the substance of which these luminaries are composed was at first in a gaseous state; which constituted a chaos. All the bodies, whether simple or compound, which have concurred in the formation of our planetary system, and of the earth in particular, must, at this epoch, have had a temperature higher than that at which the least volatile of all their substances would remain in a liquid state. That liquid or solid bodies should be formed from this immense gaseous mass, it was necessary that it should have cooled down; and the first deposition would take place, when the temperature descended to that point when the least volatile amongst them would cease to subsist in the state of an elastic fluid. When all this first sort of matter, furnished by a determinate portion of the vast expansive mass, had united in a single liquid body, (a mass which, in virtue of the mutual attraction of all its parts, must have taken the form of a flattened spheroid, if it revolved upon itself), there would be no new deposition, until, by the continuation of the refrigeration, the mass had descended to a temperature at which a second gaseous substance, on being condensed, would become liquid. On this happening, this second substance would be deposited upon the first, around which it would form a concentric layer. By the continuation of the cooling process, the other gaseous substances would successively come to be deposited.

Each of these depositions would probably be formed of one single substance, whether it was simple or compound; for it is difficult to admit, and we find no examples of it, that two different substances become liquid at precisely the same degree of temperature. These substances thus deposited in regular and concentric layers, must have probably acted chemically the one upon the other. Hence would result the formation of combinations, disruptions, the increase of temperature, elevation of the surface by a kind of ebullition, and finally the formation of solid matter, whensoever one of the compound products required a very elevated temperature that it might remain in a liquid state. It would only be after a great number of convulsions, and in virtue of a subsequent refrigeration, that a crust could be formed sufficiently solid to present an obstacle to new chemical combinations. But when the temperature should have been so far depressed as to allow a new substance to come and rest in a liquid state upon this solid bed, phenomena analogous to those we have been alluding to would occur, either on this bed, if it were of a suitable character, or on some inferior one, which the liquid substance might reach through some fissures in the first. New disruptions and explosions must have resulted from this, which would, more or less, break the exterior solid crust.

Thus may we assign a reason for the successive revolutions which the earth has experienced, and for the breaking up, and the disposition in all sorts of inclination, of beds at first formed in lines parallel to each other.

We shall not follow the author in the details he supplies in explaining the different catastrophes or cataclysms which the surface of the earth must have undergone, and of the order in which the different products of nature, whether vegetable or animal, may have succeeded each other. We have already said enough to show, which was the object we had in view, that the existence of a solid nucleus is in no respect incompatible with the manner in which we may suppose that the globe has been formed, and may very well be reconciled with its being flattened at the two poles.

We shall only further add, that at a certain epoch of the refrigeration, water must necessarily have deposited itself upon the whole of the solid crust of the globe, though, at the termination of the catastrophes and elevations which have resulted from it, this water now covers only a portion of the surface of the earth. It is this water which, now penetrating to the metallic nucleus, and exerting a most active chemical agency on its surface, produces the heat, the electrical currents and volcanoes. Davy has already shewn that the phenomena which accompany volcanic eruptions, as well as the properties of gases which escape from volcanoes, are altogether favourable to this opinion. Latterly M. Boussingault, at the termination of his chemical researches upon the nature of the elastic fluids which are disengaged from the volcanoes near the equator, has found that these fluids are the same in the different volcanoes, namely, that they consist of watery vapour in very large quantity, of carbonic acid, and hydro-sulphuric acid gases, and sometimes of sulphureous vapours. The presence of hydrogen shews, that there must have been decomposition of water, and that of sulphur, that this substance is found mixed, on the surface of the metallic nucleus, along with the highly oxidable metals of which the nucleus is probably formed. This union is still more natural, inasmuch as the temperatures at which sulphur and these metals become solid very nearly approximate. An analysis of the mineral springs of the Cordilleras, for which we are also indebted to M. Boussingault, has proved that they contain sulphuretted hydrogen gas, as in truth, nearly almost all mineral springs do; which seems to demonstrate that they proceed from vapours and gases which, arising from the surface of the affected metallic nucleus, find their way through the fissures, and condense themselves in the cavities which the mountains contain, whence finally they flow into the plains. The presence of carbonic acid, which almost invariably accompanies the sulphuretted hydrogen gas, is probably owing to carbonates, which lie in the neighbourhood of the bed that is attacked, and which are decomposed by the strong heat which the chemical action generates.

The result of the preceding short exposition, viz. that there exists a chemical action which is going forward at a certain depth beneath the surface of the earth, seems thus to be demonstrated. Hence not only volcanoes, and earthquakes, and mineral springs, and the increase of temperature as we penetrate into the oxidised crust, become of easy explication, but the production of electrical currents becomes a necessary consequence of the same chemical action. As to the general direction of the currents, it will depend upon the relative position of the negative elements, which are the substances already oxidated, and the positive elements, which are the metals of which the surface of the nucleus is formed; and as this position may change, we can understand how the variations in the direction of the needle would necessarily result. The earth's rotation, and its flattened form, which would produce at the two poles a closer contact betwixt the oxidised crust and the metallic nucleus, are circumstances which would also produce a necessary influence upon the direction of the terrestrial currents. In conclusion, we may add, that the existence of these currents is no longer purely hypothetical, since different English naturalists, and first and especially Mr Fox, have succeeded, by means of the galvanometer, in discovering evident traces of them in the metallic veins of the mines of Cornwall.

GEOLOGY OF THE VALLEY OF OODIPOOR. *By JAMES HARDIE, Esq. Bengal Medical Establishment, Member of the Asiatic Society, &c. Communicated by the Author.* (*Concluded from* p. 59. vol. xv.)

About two miles to the north of the bund of the Oodisagor, the barrier range is traversed by another barrier or cross valley, the entrance to which is guarded by the Dubaree gate. Proceeding west from this position towards the city of Oodipoor, about nine miles distant, we have first a continuation of the quartz-rocks of the Oodisagor bund. The truncated edges of the nearly vertical quartz strata constantly protrude, as we follow the course of a steep and rocky path, which traverses a low ridge connecting the hill ranges on our right and left. On the western slope of this ridge the clay-slates or argillaceous schists first make their appearance; and the quartz-rocks, as we approach the spot, partake more and more of an argillaceous character. The strata which now present themselves consist of argillaceous and calcareous schists, which form frequent alternations with each other. The calcareous or rather limestone schists, are of a reddish-brown colour; they effervesce strongly with acids, are more or less schistose, inclining to fine schistose. Their internal texture is fine angulo-granular; they include numerous scales of mica, and isomorphous grains of a greyish-coloured limestone are occasionally intermixed with others of a reddish colour. The argillaceous schists are generally soft and sectile; their colour is different shades of grey; they are generally fine schistose, include numerous scales of mica, and frequently form a rock, which may be described as intermediate between clay and mica slate. In some of the varieties, occasional laminæ of calcareous spar are observed; these last are the *calschistes* of the French. Proceeding west we have still argillaceous schists of different kinds, alternating with talco-argillaceous schists, silico-argillaceous schists, and occasionally with quartz-rocks similar to the quartz of the barrier range. The route lies over an uneven rocky plain, with alternate low ridges and hollows, corresponding with low ranges observed on our right and left, which are separated from each other by narrow longitudinal valleys. We then enter upon the irregular mammillated plain, constituting what is usually termed the Valley of Oodipoor.

To describe all of the modifications of argillaceous schist which we observed on the route we have just traversed, would extend this paper to an undue length. On referring to a descriptive catalogue of specimens from this part of the country, deposited by me in the Museum of the Asiatic Society of Calcutta, I find that the varieties are very numerous, and that I have arranged the principal under one or other of the following heads; namely, talcose schist (found only in one locality),—argillaceous schist,—rocks intermediate between talcose and argillaceous schist,—between chlorite and argillaceous schist,—between micaceous and argillaceous schist,—between quartz-rock and argillaceous schist,—and between greenstone and argillaceous schist. Some of the argillaceous schists approach to the nature of roofing slate, but this is rare. The talco-argillaceous schists are the most numerous; these are generally soft, have a soapy feel, more or less prominent, a lustre inclining to silky, and a fine schistose texture. The micaceous-argillaceous schists are also common, and rocks allied in their nature to chlorite-schist are by no means rare. The strata as yet observed are arranged in a nearly vertical position, and though some, especially of the softer kinds, exhibit partial bendings, their general course may be stated to be rectilinear. The strata of the plain and lowlands are, on the other hand, much distorted. It is not generally practicable to trace for any distance the rocks which now present themselves. The surface for the most part is covered either by the kunkur beds described in my last, or by the cultivated soil of the valley. We have an opportunity, however, of forming a tolerably correct estimate of the nature and mode of occurrence of the rocks in this position, by pursuing the course of the Bedus, in the channel of which are exposed outcroppings of a great variety of strata. Greenstones and greenstone schists are now observed alternating with the argillaceous schists. The greenstones resemble those of the trap formation; they are of a dark colour, frequently almost blackish, have a fine porphyritic texture, and occasionally include crystals of calcareous spar. They are either laminated in strata, generally of considerable thickness, or, like the granites, they assume the form of regular prismatic masses, alternating with other stratified rocks. The greenstone schists pass insensibly into the above; they have a schistose texture, more or less fine, and bear precisely the same relation to the greenstones that the gneisses bear to the granites.

Intimately connected with the above, and in many situations occupying their place, occur a series of isomorphous rocks, which may be described as greenstones which have lost their hornblende. They are of a greyish colour, have a mottled or rather minutely porphyritic aspect, and a basis of the nature of compact felspar, through which minute grains of quartz are dispersed. The argillaceous schists with which the rocks just described alternate, are similar in form and variety to the argillaceous schists of the boundary ranges. A common kind is of a bluish colour, with a rather earthy aspect, and a fine schistose texture. The colour in this respect seems to depend on copper, which may be detected on digesting fragments of the rock in nitric acid, and afterwards plunging a rod of iron into the solution thus obtained. Another variety is of the nature of greywacke schist, as commonly described; it passes into a kind having a somewhat granitoidal texture, in which mica is abundant. I have said that the strata are now much distorted; great and sudden deviations from the usual line of bearing are constantly perceived; and from memoranda taken on the spot, I observe that I have noted down a number of instances of this kind, which, viewed in the aggregate, exhibit the strata as having been subjected to bendings and dislocations, which have imparted to them a local bearing in almost every possible direction. Their position is still vertical or nearly so, and, considered on the great scale, the rocks of this position may be described as forming a band or belt, which, though exhibiting many internal signs of disturbance and derangement, still follows the same general direction as the strata of the bounding formations on the right and left, *i. e.* from N.N.W. to S.S.E.

A remarkable instance of partial deviation from the usual line of bearing it may be worth while to describe. In the channel of the Bedus, about a couple of miles north of the city of Oodipoor, we have a series of rocks, consisting of argillaceous schists of a compact texture, alternating with distinctly stratified greenstones, greenstone schists, and the isomorphous rocks described as intimately connected with them. This series may be traced along the course of the river, the channel of which it crosses obliquely, and is afterwards lost under the soil. The strata are nearly vertical; they vary in breadth from a few inches to three or four feet; their direction is E.N.E. and W.S.W., and the strata seams are very distinct. The argillaceous schists have a fine schistose texture; the other rocks are characterised by cleavage lines, which occur at regular intervals, and impart to them a slaty structure larger or finer in different strata. The cleavage lines are perpendicular to the horizon, but run at right angles to the direction of the strata, *i. e.* from N.N.W. to S.S.E. This remark holds good in respect to all of the rocks of this series; the schistose structure even of the narrowest strata (which latter are very generally composed of a very fine slate of a uniform blackish colour) being at right angles to the direction of the strata seams.

Some of the greenstones exhibit the singular appearance of a *threefold* division and structure. We have, first, the regular strata seams; next the cleavage lines, at right angles to these; and, lastly, an appearance resembling horizontal stratification, which depends on the tendency of these rocks to desquamate on exposure to atmospheric influences. These phenomena are at first sight somewhat perplexing; the cleavage lines follow a direction parallel to the general line of bearing of the strata of the neighbourhood, and might easily be mistaken for strata seams: a more attentive examination, however, will at once enable us to detect the cause of fallacy. About half a mile to the westward of this position occurs a detached hill, the strata of which are of an entirely different character, and bear to the N.N.W., so that the series just described does not appear to be continued far in a westerly direction. On the contrary, from occasional outcroppings of the strata forming it, we learn, that, in the southern portion of the valley, it acquires a line of bearing more in conformity with the other strata of the neighbourhood. The phenomena of the cleavage lines I have not seen in any other situation in the Valley of Oodipoor *; and it is somewhat remarkable, that, in the instance in which the strata are observed to deviate the most from the usual direction, the cleavage lines, as if to compensate for the irregularity, should acquire a direction parallel to that which the strata might *a priori* have been expected to follow.

The hill ranges which bound the Valley of Oodipoor to the westward, are similar in form and constitution to the eastern barrier ranges. Immediately to the south of the city we have a longitudinal rocky hill, distinguished by a remarkably sharp spire or crust, which in its course from south to north describes a curve rising gradually in the former direction, and again descending towards the north, where it is separated from the low ridge of the city by the deep and rugged chasm across which the principal *bund* of the Puchola lake has been thrown. The lake is situated in a narrow longitudinal valley, between the ridge just alluded to and another sharp crested range to the westward. The city ridge is composed of a quartz-rock similar to that of the Oodisagor *bund*; near the base on either side, the quartz passes into and alternates with the argillaceous schists. The range to the westward of the Puchola is composed of argillaceous schists passing into greenstone schist; they are of a uniform and unfissured texture, and can easily be cut into slabs of considerable length, which are used in place of beams in roofing.

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* Similar appearances are by no means uncommon in Rajpootana. At Deosa, about forty miles east of Jepoor, there is a remarkable insulated hill, composed of strata of semitransparent quartz, a very straight layered gneiss. and a gneiss of a peculiar waved aspect, which form frequent alternations with one another; and all these are characterized by cleavage lines placed at right angles to the strata seams. Some of the gneisses have a tendency to acquire a concentric laminar structure, and, in consequence of this tendency, individual strata, where exposed at the surface, have acquired throughout an apparently cylindrical form. There are other peculiarities in the stratification of this hill which I have no space to allude to in this place.

The strata forming the two ranges just described do not bear precisely in the same direction. The eastern range with its strata stretches N. and by E., and S. and by W. The western range, on the other hand, bears N.N.W. From this arrangement, it follows that the Valley of the Puchola has a somewhat triangular shape, as if it had been formed by a disruptive force operating on the principle of a wedge. The eastern range, if produced towards the south, would meet its fellow of the opposite side: in this direction, however, it terminates rather abruptly. Similar phenomena I have observed as characteristic of many other of the narrow valleys; and I believe that traces of the same kind of arrangement may be detected in the distribution of the principal hill ranges of this portion of India. To this subject I may again revert.

The ridge on which the city stands merges, at the northern extremity of the Puchola, into a broad tabular range of similar altitude. This range corresponds with the axis of the valley; and the strata, as they emerge from beneath the surface of the water, follow a course nearly N. and S. The strata are of quartz-rock, flanked on either side by argillaceous schists; and towards the centre they exhibit marks of rupture and dislocation. This range is continued for about a mile to the north of the city, where it terminates abruptly, its base being watered by a small *gil* or tank, surrounded by a tract of low marshy ground. A short distance to the north of this position we have the detached hill, mentioned as situated to the west of the series of strata described above as remarkable for the extent of their deviation from the usual line of bearing. The hill in question is ridge-shaped, and from its position seems to be an out-lier of the tabular range just alluded to. The rocks composing it, however, are of a different character; the strata bear N. and S., with a slight tendency to the W. of N. They consist of a very hard siliceous magnesian limestone, alternating with a granular quartz. The limestones are of a bluish or greyish colour, of a fine granular or crystalline texture, and are feebly translucent at the edges. They are traversed by numerous thin laminæ of white quartz. The limestone has a tendency to acquire, on exposure, a blackish colour; and its weathered surface exhibits the quartz laminæ protruding in numerous plates, giving rise to a sort of honeycombed or reticulated appearance. The quartz laminæ sometimes alternate with layers of the darker-coloured limestone —thus exhibiting a structure somewhat analogous to that of gneiss. When a portion of this rock is plunged in dilute nitric acid, it dissolves slowly with effervescence, the quartz laminæ are left unchanged, or slightly corroded on the surface, while the silica which was in combination with the earthy carbonates is precipitated in the form of a fine powder. Carbonate of magnesia is associated in considerable proportion with the carbonate of lime, but the rock does not afford a lime capable of forming a mortar. Iron and manganese may be detected by the usual reagents in its composition *.

Associated with and passing into the above occurs a rock of a coarser angulo-granular texture, composed of quartz and felspar, the latter either the common or the glassy variety. This rock is internally of a greyish colour, its weathered surface being of a rusty brown. Mica in minute scales is of rare occurrence, and rhomboidal crystals of calcareous spar are also occasionally present. This rock passes into a quartz similar to that described as occurring in the ranges to the south; and, as the strata of the latter are in the direct line of bearing of those which now occupy our attention, and as both preserve the same relative position to the argillaceous schists by which they are flanked, we may perhaps conclude that they form but modifications of the same rock considered geologically. In other positions I have observed differences equally great in the internal texture and composition of strata which seem to form one con-

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* During the very imperfect analysis which my limited means enabled me at the time to institute, some rather anomalous results, depending on metallic combinations, were obtained. I could not, however, depend on the purity of the reagents which I possessed; and as I had no opportunity of procuring others, and no means of preparing them, I mention the circumstance in this passing way, in the hope that some other observer, whose duties may lead him to that quarter, will undertake the analysis. The hill where the rock occurs will be at once distinguished by a small Hindoo temple erected on its summit.

tinuous series; and the merging of the quartz strata, more especially, into the coarser-grained rock just alluded to, is by no means uncommon. It must, at the same time, be borne in mind, that, upon the principles of the elevation theory, different sets of strata, not originally continuous, may have been upheaved along the same line of fissure, and thus give origin to the phenomena adverted to.

In my next I shall attempt a description of the mineral veins of the Valley of Oodipoor, and shall afterwards, if space permit, proceed to examine a few other geological phenomena, which may assist us in forming a more correct estimate of the natural history of the formations treated of.

ON THE ANIMALS DEPICTED ON ANTIQUE MONUMENTS. *By M. MARCEL DE SERRES.* *(Concluded from page 175.)*

THE *vertebral* animals already enumerated * are not the only ones which attracted the attention of the ancients: the *invertebral* also received consideration. Among the *Mollusca* they have depicted and described the common pulp (*Sepia octopodia*); the cuttle-fish (*Sepia officinalis*); and the oyster (*Ostrea edulis*). Some of the *Crustacea* are also found figured on antique monuments; principally the common crab (*Cancer mænas*), and the common varieties of lobster, viz. *Palinus quadricornis*, *Astacus marinus*, *Squilla fusca*, or *Cancer squilla*, and finally the crawfish (*Astacus fluviatilis*).

The *insect tribes* are frequently depicted, and in considerable variety. Some kinds are even modelled upon stones, distinct in character, and with great fidelity. The sacred beetle is the most conspicuous of these, most probably from the great veneration in which it was held. But other *coleoptera* have also been figured, and some of these seem to be connected with *Cetonnia* and *Dermestes*. Many grass-hoppers (*Gryllus locusta*) are likewise found on these monuments, as also some *Hymenoptera*, *Diptera*, and *Aptera* of the tribes *formica*, *vespa*, *apis*, *tabanus*, *asilus*, *musca*, and *scorpio*. The varieties of these different tribes are those that are the most common, and the most generally spread, over those temperate regions of our globe.

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* The list of vertebral animals described by the ancients, given by M. Marcel de Serres, will be inserted in our next Number.

Several Lepidoptera, remarkable alike for the beauty of their hues and their great agility, have commanded the attention of the antique artists. In studying the originals, we peculiarly observe, in the representation of these creatures, so light and brilliant, that genius of imitation which so eminently characterises the Greek and Roman schools, especially during that splendid epoch, when these schools, adhering to true principles, produced those *chefs d'œuvres* which even yet astonish us.

The facts which we have now recorded, will undoubtedly be sufficient to prove to every unprejudiced individual, the minute accuracy the ancients had attained in the imitation of all natural objects. If a strong bias towards that which was true has always guided them when they composed the fantastic creatures of their fancy, inasmuch as each of the parts going to make the whole is, when taken separately, minutely accurate, we may safely conclude, that a not less minute attention has been given in the representation of the real beings of which they wished to communicate a just and precise conception. To be more assuredly convinced, we have only to glance generally over the antiques that remain of the purer periods of Greece and Rome, and, to a certain point, of Egypt too. Modern artists have almost universally granted this meed of praise and justice to their brethren of antiquity.

In thus considering their works in a point of view that is altogether peculiar, and perhaps hitherto not perceived, we have made this truth still more striking, by demonstrating that their most fantastic productions, in each of their component parts, are a faithful and accurate imitation of nature; the whole is as monstrous as it is fantastic, but the details remain accurate as they ought to be.

This point being proved, it now remains for us to establish that the antique monuments preserve the traces of species which no longer appear to be found on the globe, and which, therefore, must have been lost and become extinct since the epoch of history. Thus we have species which have become extinct in consequence of geological phenomena, others whose destruction may have been contemporaneous or posterior to man's creation; and finally, others still, whose races have become extinct within the records of history.

It is necessary to admit the last of these conclusions, inasmuch as the antique monuments exhibit the figures of animals which are quite different from any of our living varieties. These animals, then, must have been lost, like those which, buried in the bowels of the earth, have no longer representatives existing on it.

In truth, that we may come to an accurate conclusion, it is necessary to admit (what we conceive we have proved) that the ancients observed nature with the utmost exactitude; and besides, that amongst the animals depicted by them, and which have no longer representatives on the earth, there are those the organization of which rendered their existence possible. We shall prove this second proposition ere long, at the same time showing, that the animals represented on the antique monuments differ less from our present existing species than certain fossil species, or than several of those which recently have been discovered in that continent which had so long eluded the discovery of our navigators.

Nothing, then, ought to prevent us from considering them as real species, which have existed at a former time. We might, perhaps, have continued in doubt, had they exhibited an organization as singular and absurd as that of the gigantic Megalosaurus, and the extraordinary Ichthyosaurus and Plesiosaurus; because it might have happened, that, on the unsupported evidence of the monuments of ancient Egypt and Rome, we should not have admitted the existence of a lizard as large as a whale, and still less that of an animal half reptile and half fish. A reptile, with four feet, with a neck longer than its body, would have appeared to us no less problematical; and very likely we should have rejected it also as a creature of fancy, the production of the vivid imagination of the artists of antiquity. But who now doubts of their former residence on the earth, and of their reality? so that, before we can deny that an animal has existed, we must consider, not only if its organization be extraordinary, but also if it satisfies the conditions of existence to which it is subjected.

With respect to the Megalosaurus, the Ichthyosaurus, and the Plesiosaurus—the subjects we have chosen for examples—it may perhaps be objected, that we have introduced specimens, the existence of which, anterior to that of man himself, belongs, so to speak, to a scarcely finished world, the ruder productions of which could not survive the various changes to which the globe has been subjected. It might be said, that the species contemporaneous with man, are the only ones which can be compared with our living races, and that their tribes have nothing so uncommon nor so extraordinary. Belonging to the stable period to which the globe has now arrived, more especially since the times of history, these have in themselves a principle of regularity which insures their existence, by giving them the means to withstand the various causes which would tend to their destruction.

Even allowing there might be something in this objection, still other examples, and not less remarkable, will ere long prove, that when antique monuments exhibit the representation of species in themselves possible, we ought not to regard them imaginary, although we can no longer discover traces of them on the globe.

In short, would it have been right for us to reject as a fable the terrestrial quadruped which the monuments of antiquity alone had represented as having the beak of a palmipede bird, because we had fancied that such characters could not be combined? In doing so, we would have contradicted nature, which, in that continent so long unknown, has brought such an animal to light, along with many other not less remarkable productions. And if descriptions had been left us of this remarkable being, should we have been justified in regarding them apocryphal, because they announced that this quadruped with a bird's bill, had this other peculiarity of the animals of this class, that, namely, of depositing its eggs?

We might with as much propriety reject the account of Aristotle, when he describes the history of that fish (*Gaubius niger*, Lin.) which makes a nest, and hatches its eggs after the manner of birds. And yet, very recently, Olivi, in examining this same fish, has confirmed the observations of Aristotle; and that which most strikingly proves the accuracy of that great man is, that Olivi has not a doubt that Aristotle, under the name of Phycis, has accurately described the habits of the Gaubius *.

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* In a former Number of this Journal, there is a notice of fishes' nests, with their ova, observed on the east coast of Scotland.

If we had rejected the ornithorhyncus as fabulous, so probably should we have done with the echidna and the kangaroo, animals as paradoxical as it,—also inhabitants of New Holland. In like manner, if some artist of Herculaneum or Pompeii had amused himself with giving black wings to the swan under whose form Jupiter seduced Leda, antiquaries, perhaps, would have recognised nothing more than an ingenious allegory, and never suspected that whiteness was not a necessary character of the swan. And yet we know that New Holland has given us black swans, as well as terrestrial mammiferæ with a bird's bill. Nor in this is there any thing very surprising, nor contrary to the laws of nature. Hence, then, before concluding that a species which we do not now see has never existed, it is necessary to be well satisfied whether its organization is compatible with existence or not. We may also conclude, from these facts, that since simple and natural causes may easily operate to the destruction of rarities which we suppose to be lost, it is not at all necessary to have recourse to violent revolutions, beyond the ordinary course of events, to account for their disappearance.

In a succeeding memoir, we shall solicit the attention of geologists to those animals which are figured on the monuments of antiquity, and which appear now to be destroyed. To this may succeed another, in which we shall discuss the question, How far are we acquainted with all the rocks and minerals employed by the ancients in the construction of their monuments?

Does it, in the mineral, ever happen, as in the other kingdoms of nature, that there are species which have disappeared from the surface, and which are not now found in any of our mountain ranges? This could only happen by the elevation of mineral masses, portions of which might cover minerals originally near the surface, and thus remove them from our view. This disappearance, then, if it exist at all, has been produced by causes wholly different from those which have operated in the destruction of living species. The facts connected with this enquiry are of the most interesting character, since they lead to the supposition that there have been considerable elevations of solid masses within the times of history, nearly in the same way as the volcanic fires have thrown to the surface of the earth minerals, which, perhaps, but for these eruptions, we might never have seen.

OBSERVATIONS DURING A VOYAGE FROM ENGLAND TO FORT VANCOUVER, ON THE NORTH-WEST COAST OF AMERICA. *By Dr* MEREDITH GAIRDNER. *In a Letter to Professor* JAMESON [*].

I SEIZE the first opportunity which has presented itself since my arrival on the shores of America, of communicating to you a few short notices of the scenes which have passed under my view since quitting my native country.

Our voyage hither was long, and, by many, would have been considered as tedious; but the atmosphere which surrounded us, and the ocean which we were traversing, presented so many interesting and continually changing objects to our view, that I am tempted to think what is called the *ennui* of a long sea-voyage, is owing rather to the temper of mind of the voyager than to the circumstances in which he is placed. Daily I had a constant source of occupation in noting the oscillations of the barometer (we had a very good marine one on board, constructed by Gilbert), the diurnal changes in the temperatures of the air, and the superficial strata of the ocean; in carrying on a series of astronomical observations for the determination of the ship's place, in concert with our navigators; and in observing the habits and structure of the different aërial and aquatic animals which fell in our way in the course of our long traverse. And yet I had daily to regret the omission of many highly interesting observations, from the want of opportunity, and the means of their performance. A merchant vessel was not the most favourable place for such observations. I regularly noted the height of the barometric column at 9–10 A. M., noon, and 4–5 P. M., besides different observations at shorter intervals on particular occasions. You will form a better idea of these from the following Tabular View, which I drew up at the conclusion of the voyage. I confine it to the two torrid zones, as there the atmospheric phenomena proceed with a regularity unknown in temperate latitudes.

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[*] "Fort Vancouver is situated in a plain, about ninety miles from the mouth of the Colombia River, on its north bank. In my journey of about fourteen days into the country of the Walamet River, a tributary of great size which falls into the Columbia opposite to Fort Vancouver, I remarked this country as consisting of a series of extensive plains or prairies, covered with grass, interspersed with belts of fir and oak, and bounded on the east by the maritime chain of snowy peaks, and on the west by the ranges immediately skirting the Pacific. The thick cover of soil prevented my obtaining much information in my favourite geognostic science; but being my first essay at voyaging in NW. America, was highly interesting. One of the most interesting objects which I saw in these plains, were large blocks of granite scattered on the surface, which must have come from a great distance, as this rock is not found fixed in the maritime chain or country between it and the sea-coast to the south of the Colombia,—a tract, as far as I have seen specimens, or have been able to procure information, consisting almost if not entirely of igneous rock from modern cellular lava (as at the falls of the Walamet) or compact block basalt.

"One of the most striking phenomena in the atmospheric constitution of Fort Vancouver, is the extent of the diurnal change of temperature, 35° from sunrise to 2 P. M. is not uncommon. Once I saw the thermometer (Fahr.) at 43° at sunrise, while at noon of the same day it stood at 87°. The highest I ever saw it was one day in July, when at 2 P. M. it stood at 89° in the shade."—*Extract from Dr Gairdner's Letter to his father, dated* 31*st August* 1833.

Date, and Place of Observation.		Mean.	Obs. of Noon.	Range, 9 A. M. to 4 P. M.
I. North Atlantic Torrid Zone.	1832,			
	Oct. 13.	30.09	30.08	0.08
	... 14.	30.09	30.07	0.05
	... 15.	30.09	30.09	0.06
	... 16.	30.09	30.16	0.18
	... 17.	30.02	30.02	0.13
	... 18.	29.99	29.98	0.13
	... 19.	30.05	30.05	0.04
	... 20.	30.03	30.03	0.03
	... 21.	30.04	30.06	0.08
	... 22.	30.01	30.04	0 04
	... 23.	30.05	30.05	0.08
	... 25.	30.04	30.02	0.09
	... 26.	30.03	30.05	0.07
	... 27.	29.98	29.96	0.10
	... 28.	29.95	29.97	0.03
	... 29.	30.03	30.04	0.16
	... 30.	30.06	30.05	0.04
	... 31.	30.07	30.08	0.08
	Nov. 1.	30.02	30.03	0.08
	... 3.	29.97	29.98	0.10
	... 4.	29.99	30.01	0.08
	... 5.	29.97	30.01	0.09
	... 6.	29.91	29.91	0.13
	... 7.	29.91	29.92	0.16
	... 8.	29.94	29.94	0.02
	Mean,	30.003	30.080	0.087
II. South Atlantic Torrid Zone.	Nov. 9.	29.95	29.95	0.09
	... 10.	29.88	29.96	0.17
	... 11.	29.97	29.98	0.10
	... 12.	30.02	30.04	0.06
	... 13.	30.06	30.07	0.07
	... 14.	30.08	30.08	0.07
	... 15.	30.06	30.08	0.11
	... 16.	30.02	30.06	0.08
	... 18.	29.99	29.99	0.05
	... 19.	30.00	29.98	0.05
	... 20.	29.98	29.99	0.02
	... 21.	29.98	29.98	0.09
	... 22.	29.71	29.70	0.14
	... 23.	29.60	29.63	0.15
	... 24.	29.68	29.67	0.02
	Mean,	29.895	29.905	0.063

Date, and Place of Observation.		Mean.	Obs. of Noon.	Range, 9 A. M. to 4 P. M.
III. South Pacific Torrid Zone.	1833,			
	Feb. 16.	30.15	30.16	0 14
	... 17.	30.07	30.04	0.10
	... 18.	29.95	29.98	0.03
	... 19.	29.96	30.03	0.15
	... 20.	30.03	30.05	0.10
	... 21.	30.05	30.10	0.10
	... 22.	30.01	30.04	0.07
	... 23.	29.92	29.98	0.11
	... 24.	29.97	29.97	0.08
	... 25.	29.89	29.90	0.01
	... 26.	29.97	29.94	0.04
	... 27.	29.96	29.97	0.07
	... 28.	29.94	29.96	0.23
	Mar. 1.	29.88	29.90	0.11
	... 2.	29.91	29.88	0.26
	... 3.	29.91	29.95	0.14
	... 4.	29.93	29.98	0.08
	Mean,	29.965	29.980	0.13
IV. North Pacific Torrid Zone.	Mar. 5.	29.93	29.89	0.06
	... 6.	29.96	29.98	0.10
	... 7.	29.95	29.96	0.10
	... 8.	29.92	29.94	0.11
	... 9.	29.95	30.00	0.12
	... 10.	30.01	29.99	0.06
	... 11.	29.94	29.97	0.07
	... 12.	30.00	29.98	0.03
	... 13.	29.91	29.93	0.18
	... 15.	29.95	30.01	0.14
	... 16.	29.98	29.97	0.05
	... 17.	30.00	29.99	0.12
	... 18.	30.04	30.01	0.15
	... 19.	30.02	30.02	0.09
	... 20.	30.09	30.07	0.09
	... 21.	30.11	30.10	0.09
	... 22.	30.18	30.20	0.14
	... 23.	30.15	30.18	0.08
	... 24.	30.20	30.21	0.08
	... 25.	30.14	30.13	0.12
	... 26.	30.19	30.19	0.07
	... 27.	30.25	30.26	0.02
	Apr. 10.	30.28	30.33	0.09
	Mean,	30.093	30.103	0.083

From which:

North Pacific Torrid Zone,	Mean,	30.093
South Pacific Torrid Zone,	......	29.965
Mean of North and South Pacific,		30.029

North Atlantic Torrid Zone,	Mean,	30.003
South Atlantic Torrid Zone,	......	29.895
Mean of North and South Atlantic,		29.949

I was chiefly led to the calculation of these barometric means, by the idea, that *perhaps* a difference of level between the two oceans might be the result. May not the irregularities resulting from the motion of the ship, winds and weather, be lost in the number of partial observations, giving errors both in excess and defect? If they are of any value in this point of view, they would indicate the level of the Pacific to be lower than that of the Atlantic Ocean. To find the amount, using Dabuisson's Portative Table, given in his Geognosie, i. 467.:

30.029 In.	= 768.7 mill^{s}.	= elevation,	76
29.949	= 760.7	=	97
		Difference,	21 metres = 69 feet,

neglecting all corrections for temperature, &c. as these will operate in the same direction, and nearly about the same amount, on both barometric columns. *N. B.* The cistern of the barometer used was about 10 feet above the level of the water.

My observations on the temperature of the surface strata of the ocean were daily made throughout the voyage at 7–8 A. M., noon to 2 P. M., and sunset. It would occupy too much space to give here the detail of these observations. I shall therefore merely give you their abstract, following two methods, by comparing which, more correct conclusions will be formed than by either method singly.

Method 1.—Where the temperatures given are the means of the observations on the days when the parallels indicated were crossed.

ATLANTIC OCEAN.					
Latitude.	Longitude.	Temperature,—Fahrenheit.		Sun's Meridian Altitude.	Date.
		Sea.	Air.		
50° N.	4° W.	58.6°	58.7°	41°	1832, Sept. 20.
45	11	63.5	66.0	44	... 24.
40	15	66.6	66.3	47	... 29.
35	16	71.7	68.0	51	Oct. 2.
30	19	73.6	72.0	54	... 7.
25	22	74.4	73.5	58	... 11.
20	24	76.3	75.6	62	... 14.
15	26	79.6	78.1	65	... 16.
10	23	81.6	79.1	69	... 21.
5	17	80.1	78.7	70	Nov. 3.
0	23	76.2	76.7	73	... 8.
5 S.	27	76.7	77.8	77	... 11.
10	31	79.2	79.1	82	... 14.
15	34	80.1	76.6	86	... 19.
20	37	76.6	75.3	90	... 22.
25	37	72.1	73.4	85	... 25.
30	38	68.9	67.8	81	... 29.
35	49	70.9	71.6	77	Dec. 7.
40	54	63.8	56.2	74	... 13.
45	54	56.4	55.9	68	... 17.
50	56	45.9	48.6	63	... 21.
55	64	43.2	45.6	55	... 29.
58	67	41.3	42.0	55	1833, Jan. 2.

Method 2.—Where the temperatures given are the mean of all the observations on the days the ship was within the limits of the zone indicated.

ATLANTIC OCEAN.					
Zone.	Date.	Limits of the Zone.		Mean Temperature of the Zone.	No. of Obs. from which the Mean is calculated.
		Latitude.	Longitude.		
N. Lat. 45°	1832, Sept. 21. to 28.	49° to 39°	5° to 15° W.	63.7°	24
... 40	... 25. to Oct. 1.	44 ... 34	12 ... 14	66.9	21
... 35	... 30. to ... 6.	39 ... 29	14 ... 18	72.2	21
... 30	Oct. 3. to 10.	34 ... 26	17 ... 21	73.6	24
... 25	... 8. to 13.	29 ... 21	19 ... 23	74.4	18
... 20	... 12. to 15.	24 ... 16	22 ... 25	76.2	12
... 15	... 15. to 20.	19 ... 11	25 ... 23	80.2	18
... 10	... 17. to Nov. 2.	14 ... 6	25 ... 17	80.8	51
... 5	... 22. to ... 7.	9 ... 1	23 ... 22	80.0	51
Atl. Eq. Zone,	Nov. 4. to 10.	4N.–4S.	18 ... 26	77.5	21
S. Lat. 5°	... 9. to 13.	1 ... 9	24 ... 29	77.3	15
... 10	... 12. to 18.	6 ... 14	28 ... 31	79.3	21
... 15	... 15. to 21.	11 ... 19	32 ... 34	79.9	18
... 20	... 20. to 24.	16 ... 24	35 ... 37	76.4	15
... 25	... 23. to 28.	21 ... 29	36 ... 35	71.3	18
... 30	... 26. to Dec. 6.	26 ... 34	36 ... 47	68.5	33
... 35	... 30. to 12.	31 ... 39	40 ... 53	69.1	39
... 40	... 8. to 16.	36 ... 44	50 ... 55	65.4	27
... 45	... 14. to 20.	41 ... 49	53 ... 57	54.2	21
... 50	... 18. to 28.	46 ... 54	57 ... 64	46.2	33
... 55	... 22. to Jan. 2.	51 ... 58	57 ... 67	43.8	36

South Pacific Ocean.

Zone.	Date.	Limits of the Zone.		Mean Temperature of the Zone.	No. of Obs. from which the Mean is calculated.
		Latitude.	Longitude.		
S. Lat. 55°	1833, Jan. 3. to 19.	58° to 51°	68° to 79° W.	43°.3	51
... 50	... 15. to 26.	54 ... 46	76.. 82	50.4	36
... 45	... 21. to 31.	49 ... 41	78...86	55.4	33
... 40	... 30. to Feb. 2.	44 ... 36	86...87	61.7	12
... 35	Feb. 1. to 7.	39 ... 31	86...88	69.6	21
... 30	... 5. to 13.	34 ... 26	88...87	74.2	27
... 25	... 10. to 17.	29 ... 21	87...92	75.8	24
... 20	... 15. to 20.	24 ... 16	89...97	75.6	18
... 15	... 19. to 23.	19 ... 11	95..101	76.5	15
... 10	... 22. to 26.	14 ... 6	99..106	77.7	15
... 5	... 25. to Mar. 3.	9 ... 1	104..113	79.1	21
Pac. Eq. Zone,	.. 28. to 9.	4S.–4N.	109..121	79.8	30

South Pacific Ocean by Method 1.

S. Latitude.	Longitude.	Temperature,—Fahrenheit.		Sun's Meridian Altitude.	Date.
		Sea.	Air.		
55°	77°	43°.1	42°.8	55°	1833, Jan. 14.
50	78	50.7	49.9	60	... 20.
45	85	56.2	56.8	62	... 27.
40	86	60.1	60.7	66	... 31.
35	88	69.6	65.0	71	Feb. 3.
30	87	73.6	71.7	74	... 8.
25	88	76.2	75.7	77	... 14.
20	94	75.2	73.7	81	... 18.
15	98	76.7	76.6	85	... 21.
10	103	77.5	78.2	89 N.	... 24.
5	107	79.3	80.4	87 S.	... 27.
0	114	78.7	79.9	84 N.	Mar. 4.–5.

Since arriving here, there has been no leisure for reducing into a similar tabular form my observations in the North Pacific. The causes formerly referred to deprived me of many favourable opportunities which might have been embraced, for ascertaining the temperature of the ocean at different depths. I had several opportunities of remarking the slight effect of great atmospheric changes upon the height of the barometrical column in tropical latitudes; this was especially striking in the heavy rains that we experienced in the North Atlantic, in about 6°—8° north latitude. On one occasion, I collected 1140 grains of water in a basin of a circular form, 9.3 in diameter, in 12 hours, the barometer remaining stationary at 30.00—29.99. This was on the night of the 26-27th October 1832, Lat. 7° 20′ N., Long. 20° 3′ W.

We crossed the part of the Atlantic where Maltbrun, in his Geog. Univ. (Transl. i. 322), places the Mer de Sargasso, but not a vestige of it was seen. The 1st February 1833, was remarkable for the sudden and great oscillations of our barometer, which cannot be accounted for in the state of the winds and weather; our position at the time was in Lat. 39° S. Long. 86° W.; it would be highly interesting to connect this with any striking phenomenon in other parts of the globe, particularly in the adjoining continent of South America. On the 19th October 1832, I collected in the basin above referred to 3630 gr. of rain water, between noon and 6 P. M. This, of course, is not the true quantity of rain which fell, as some would necessarily be returned into the air by evaporation, from the exposure of so large a surface in the basin to the air.

It would be but an inadequate outline that I could give you of the impression that the magnificent constellations of the southern hemisphere produced upon me, after the eloquent delineation given by Humboldt in his Personal Narrative, which, I dare say, is fresh in your recollection, and which, I assure you, is not exaggerated. Neither will I occupy your time by dwelling upon the splendours of a tropical sunset, which has so often been the theme of voyagers, except to mention a circumstance which I do not remember to have been particularly attended to, but which, I think, may be considered as one of the strongest proofs of the remarkable transparency of the atmosphere in these latitudes; it is the violet hue assumed so regularly by the ether in the western horizon, a short interval after sunset. I have never observed a tint of the sky exactly similar in our European skies, and perhaps it may be explained in the following way: the rays of white light are decomposed into the prismatic colours, in consequence of the greater density of the air at sunset, both from accumulated vapours, and a greater length of atmospheric column that they have to traverse; and those least refrangible, such as red and orange, are what usually in northern climates alone reach the eye, while, in tropical climates, where the air is more serene, we may suppose that those more refrangible will reach the eye also; as the yellow, green, blue, indigo, violet, which agrees with the phenomena observed. We had a fine opportunity of observing the solar eclipse of Jan. 20, in Lat. 49° 38′ S., Long. 77° 53′ W. With us the app. time of immersion was 4° 30′, and of emersion 6° 44′; and the portion of sun eclipsed was 27° 30′, or about 7-8ths of his diameter, by my measurement with a sextant.

The only land of which we had a near and distinct view in the long run from England to the Sandwich Islands, was Staten Island, off the east entrance of the Straits of Magellan, and nothing can exceed its rugged and desolate aspect. Snow was lying in small patches in the sheltered hollows, near the summits of the highest hills. The summit profile of the island presented a series of denticulated eminences, rising to the north-west, to which point were generally directed all the precipices, the slope of the island being to the south-east. When the extremity of the island about Cape John was viewed with a glass, I thought I could recognise in the steep precipices, at whose base the sea broke, the outgoing of strata, with a direction S.SW. to NNE. From the regularity and smoothness of the slope of the bare rocks to the SE., and the numerous greyish white masses projecting from the general black surface of rock, I should think the prevailing rock to be mica slate, with large imbedded masses of quartz rock. Numerous vertical fissures, hollowed out by the waves into caverns, were visible along the point from Cape St. John southwards; these rose obliquely upwards to the NW., confirming the preceding idea of the position of the strata. I observed no columnar or strictly tabular masses, leading to the idea of ancient or modern volcanic productions. These observations, and some others which space prevents me detailing, were made when sailing along the coast, at about 1-2 miles distant. According to a rough calculation of the height of a mountain above Cape St. John, taking as a base the run of the ship (4 knots) for an hour, between the times of taking its angle, which I made 2580 feet, and from patches of snow existing at its summit in the midsummer of the hemisphere (Dec. 29, 1832,), the line of perpetual snow must be below 3000 feet; theoretically for Lat. 55°, it is 4900 feet. So that we have already a proof of the inferior temperature of the southern hemisphere, and yet the snow line of Staten Island must be raised, from its isolation, and its being surrounded by a vast ocean. About 500 feet of the upper part was bare rock; mosses did not extend so high, so that the superior limit of vegetation might be about 2000 feet. All these measurements are to be viewed merely as approximations; but, in regions so seldom visited by observers, I have thought that even approximations merit being recorded.

In the Atlantic, the gigantic Albatross (Diomedea exulans) of the southern seas first appeared in Lat. 30° 57′ S. on the 30th Nov., and disappeared again in the South Pacific on our reaching Lat. 35° S. During a space of two months, we had these birds more or less constantly round the ship, when their voracity afforded us all on board much amusement, as the observation of their manners was a fertile source of employment. I have seen three distinct kinds characterised by a distinct marking of the plumage; but to what extent these form distinct species, or whether they are merely varieties of sex, age, &c. would require an acquaintance with them on their native breeding places to determine. The *first* is characterised by a beautifully snow-white head, neck, and belly, with dark brown wings; it is the largest of the three, the expanse of wing varying from 10 to 13 feet. It was the most common variety before crossing the meridian of Cape Horn, being comparatively rare in the Pacific:—the *second* was all over of a dark brown colour, its head was nearly black, forming a remarkable contrast with its long white hooked beak; its size much less than that of the preceding; we were never so fortunate as to shoot or hook a specimen of this variety:—the *third* was characterised by a remarkable circular black spot on the vertex. The head, neck, and belly, were of a spotted grey colour, and the back and wings of a light brown. Its expanse of wing was about 6-7 feet; this was by far the most common variety in the Pacific Ocean. These circumstances of geographical distribution would lead us to doubt of their being mere varieties of age and sex. Of the prodigious force of wing of these birds, I have attempted giving some illustration in a letter to my Father, which you have probably seen.

We met with the Cape Pigeon on two different occasions in the Southern Ocean, in the vicinity of the American continent. And, when off the Falkland Isles, two individuals of the singular *Chionis* tribe, with a snow-white plumage, hovered for some time round the ship.

In Lat. 47° S. and Long. 57° W., we met with a shoal of a new species of Delphinus, allied in its piebald markings to the *D. leucoramphus*, Cuv. R. A. i. 291, but in which the distribution of the colours was different. Its high dorsal fin was of a black colour, which was continued from its root into a band or girdle encompassing the body. Tip of snout black, from which two black bands proceeded to a circular band, one over occiput, other along mesial line of lower jaw: ridge of back and belly behind the dorsal band, also black; all the rest of the body pure white.

The *Hydrostatic acalephæ* form an object of great interest to the voyager in the Pacific Ocean. They often form shoals of great extent; in our passage from the Sandwich Islands to Columbia, the ship sailed through one for about a week. The most beautiful of these which I saw, as well as the most curious, from the complexity of its structure, and the delicate arrangement of all its movements, was the *Physalia* or *Portuguese-man-of-war* of the English sailors, and I had an opportunity of verifying, on numerous living and dead individuals, the accuracy of the immortal Cuvier's brief notice in his R. A. iii. 285. Time would fail me, and I would run the risk of exhausting your patience, if I entered into a full detail of the many interesting phenomena observed on the Chelonia, fish of the species of the great family of Scomberoides, Squali, &c. observed on the voyage. I will merely stop to mention a fact a little curious in the physiology of the common *Bonito*, the *Scomber pelamys*, Cuv. R. A. ii. 198, (N. B. the longitudinal black bands are not by any means limited to four.) During many hundred miles of the voyage, the ship was accompanied by numerous shoals, swimming with great velocity close to the surface of the water, over the ship's quarter, and occasionally presenting to the eye their brilliant silvery bellies. When they turned for an instant on their sides, the vigour and precision of their movements was beautiful. Although packed close together, none interfered with his neighbour, although often enveloped in the foam caused by the ship, and the breaking of the waves. They seem to feel peculiar pleasure in thus coursing along in the agitated water of the ship's way; it is not from hunger, or in the expectation of food, for they will not touch the bait of a hook. What can be the attraction? The following theory suggests itselt. Fish are known to respire only the air contained in the aqueous fluid that surrounds them. Where this air is most abundant, it is probable that they will prefer resorting, as affording an increased aëration to their blood, and, consequently, more elasticity and vigour in the exercise of all their functions. Numerous bubbles of air are carried under water by the motion of the ship, affording to these fish an unusual supply of air, and forming a source of attraction round the vessel.

In sailing along the north side of Owhyhee, we had a fine view of its two snowy peaks, Monna Keah and Monna Roah, and I embraced the opportunity to take a few angles for the trigonometric determination of their elevation. Monna Keah, at a distance of 62 miles, subtended an angle of 2° 2′ 13″; and Monna Roah, at a distance of 84 miles, (Geog.) one of 1° 54′ 20″: I have inferred the distance from the difference of latitude between the ship, (by astr. obs.) and the mountains (by Vancouver's chart) and their bearings by compass, corrected for variations. From these data, I calculate the altitude of the summit of Monna Keah to be 12,081 English feet, and that of Monna Roah 15,306 feet.* Monna Keah rose in the fore ground immediately from the coast, and we had a view of it throughout its whole mass: its summit was chiefly covered by detached masses of snow, furrowed by long black vertical streaks; the snow formed a continuous surface for but a very small portion of its highest peak. The summit only of Monna Roah was seen, barely overtopping the nearer peak of Monna Keah, but easily distinguished from it by its dome-shaped round outline, and the uniform unbroken sheet of snow with which it was clad. I am inclined to place some little confidence in my measurement of Monna Roah, as, by a distant operation from the above, its altitude came out to be 15,087 feet; a difference from the former not worthy of being noticed in this method of measurement. By a direct operation, I calculated the height of the snow line on Monna Keah to be 11,256 feet, and, by another operation, 10,578 feet. The inferior snow-line was not visible on Monna

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* These numbers agree more nearly with those of Kotzebue, than of any other observer, and from what is afterwards mentioned of the snow line, I suspect are nearer to truth than the higher estimates of King, &c.

The angle is diminished 1-10th for refraction.

Roah. By a second operation, the altitude of Monna Keah came out 12,651 feet. I regretted exceedingly that the shortness of the ship's stay at Oaho, the seat of government, prevented my visiting the largest and most interesting of this group, the island of Owhyhee. This regret was rendered the more painful, by the interesting description of the island which I obtained from the American Missionaries at Oaho, who have traversed most of the island. From them I learned that the gigantic crater of Kiranea is progressively filling up, the mass of liquid lava now reaching as high as the "wide horizontal ledge of solid black lava," mentioned by Ellis in Polynesian Researches, iv. 238. Monna Roah, from an ancient, has now become an active volcano, which you will easily connect, with the above mentioned diminution in Kiranea. Dr Judd, medical-officer to the American mission in Oaho, who was in Owhyhee in Nov. 1832, informed me of a large volcano having broken out near the summit of the mountain. From the accounts the Rev. Mr Bingham had from the Rev. Mr Goodrich, who has been at or near its edge, it far exceeds in magnitude and depth that of Kiranea. Mr Rooke, surgeon in Oaho, who was in Owhyhee in August 1832, when at sea in July, on his passage thither, saw the flames issuing from this new volcano to an immense height, but, when he reached the island, the eruption had apparently ceased. Rooke, who has been on the mountain as far as the snow line, says, that its ascent is very easy, and you may ride on horseback to the very top.

Our arrival at Oaho was at an interesting juncture, in consequence of the recent death of the queen regent, Kaabawana. The islands were in a state approaching to complete anarchy, for the young king, Kaniekeouli, although acknowledged as sovereign of the whole group, had not promulgated the laws by which he meant to rule, and no efficient executive was in existence. The property and lives of the European residents (now about 200) were, consequently, entirely at the mercy of the rabble, who, however, conducted themselves in general with great propriety towards the whites, who had suffered no loss except one robbery, which was by the servant of the person robbed. All the artificial restraints imposed by the late regent being removed by her death, an opportunity was afforded for determining whether Christianity had permanently affected the minds of these islanders, or whether it was a forced state, in obedience to example and commands of their chiefs, for political purposes. A state of unbounded licentiousness certainly prevailed at the period of our visit, and some attempts were making to revive the native games, dances, &c. for many years entirely suppressed; but no one could walk through the streets of Honorara, especially on a Sunday, without being convinced that a permanent change had been effected in the religion of the island. Once, in a council of the chiefs, the question of the abolition of Christianity was started, but it was opposed by all those of greatest weight, even by the old *Neva-neva*, or high-priest himself, whose influence has suffered such a blow by the new religion, and who, consequently, might have been supposed most interested in its extinction. Till, however, the sacredness of property is secured by positive laws, these islands can make few or no advances in civilization; the question was in agitation when we were there, and it was said that an enactment to that effect was shortly to be passed.*

I took advantage of the ten days that we staid in Oaho to make a pedestrian tour through the interior of the island. The results of my observations I take the liberty of presenting to you, in the brief physico-geognostical sketch appended to the letter, as far as the island has been opened to my inspection. It is very imperfect, chiefly for three reasons: the hasty progress of my tour; the unfavourable influence of a tropical sun on geognostic pursuits; the density of vegetation, which, in many places, concealed the subjacent rock from view. But, as being the first of the kind on this interesting group of islands, it may be deemed worthy of notice. Any errors in the designation of mineralogical characters you will be able to rectify, by means of the series of specimens I now send you, per the Ganymede barque of the Hudson Bay Company.

We arrived off the bar of the river Columbia on the 1st May, and, on the 4th, reached Fort Vancouver where I have been stationed ever since. I find my situation here very different from what I was led to expect on leaving Scotland. Besides the duties of medical-officer, those of Indian-trader also devolve upon me; so that my time is so fully occupied that little or none have I been able to devote to natural history pursuits. I have made one short journey into the valley of the Walamet, one of the southern tributaries of the Columbia. But, although my opportunities of seeing North-west America have as yet been but limited, I have gathered sufficient information, and seen objects from the three kingdoms of nature, enough to convince me that it is a country of exceeding interest to the naturalist. Limestone, coal, lead, silver, gold, salt, are among its mineral riches. Among its vegetable wonders, I need only cite to you the gigantic *Taxodium* of the frontiers of California, the size of one of which, as I have from credible authority, is 32 feet in circumference, 3 feet from the ground, and 291 feet of extreme length, by actual measurement. I must reserve, however, for a future letter any information in detail I may be able to obtain regarding this region. The maritime chain contains no less than six snowy peaks, within a range of 200 miles, from the parallel of 43° to that of 46°. How often have I wished, when viewing that of Mount Hood, which towers up within 40 miles of Fort Vancouver, that it were transported to Britain, within reach of so many men illustrious in the annals of physical research *.

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* I pass over unnoticed the differences between the white residents, with the British and American Consuls at their head and the Missionaries, and refer you to the Rev. Mr Stewart's recent work, whose observations, as far as my knowledge extends, are generally correct.

FORT VANCOUVER, COLUMBIA RIVER,
August 31. 1833.

PROOFS THAT THE HUMAN BONES AND WORKS OF ART FOUND IN CAVES IN THE SOUTH OF FRANCE, ARE MORE RECENT THAN THE ANTEDILUVIAN BONES IN THESE CAVES.

M. DESNOYERS lately communicated to the Geological Society of France some observations on the human bones found in the caverns of the South of France; tending to prove that these bones, and the objects of human manufacture occurring with them, have been placed in the situations in which they have been discovered, at a period subsequent to the last great cataclysms, and that they are not cotemporaneous with the extinct species of quadrupeds with which they are associated. The first argument he brings forward to support his view of the subject, is deduced from a passage in Florus, an historian of the early part of the second century.

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* Dr Gairdner adds, "Mr Douglas, the well known traveller, arrived here a few weeks ago, from a five months' tour into New Caledonia: he was so unfortunate as to be wrecked in an attempt to descend the Fraser's river in a canoe, and to lose the whole of his collections, but he had saved his notes and instruments, and has brought many valuable materials with him."

Florus relates that Caesar ordered the crafty inhabitants of Aquitania to be inclosed in the caverns to which they had retired; in this, following a custom common to many tribes of the Celtic race, who sought in these subterraneous retreats, not only a refuge in time of war, but also a shelter from cold, magazines for their corn, for the produce of the chase and fishing, and on more than one occasion a place of concealment for the animals reduced to a domestic state. The Gauls had also, according to Caesar, great skill in subterranean excavations for the search of iron and marl; but the text of Florus is much more applicable to our subject: "*Aquitani, callidum genus, in speluncas se recipiebant, Caesar jussit includi.*" (*Florus, Hist. Rom. Epit. L.* 3. *C.* 10). These unfortunate Aquitanians must in part have perished in these caverns, and by means of the water which penetrated their bones must subsequently have become blended with the mud, the gravel, and the debris of the animals already buried in some of them and probably at a much earlier period. A stalagmitic paste would then in some places, as at Bise, cement the whole into solid aggregates; viz., the bones of the bear and the deer, some inferior layers with human bones, the fragments of earthenware vessels, terrestrial shells, the bones of the animals of modern times, and some black mud of the surface. The irregular lowering of the vault would on certain points produce a contact, and an equal adherence of the various deposits to the walls of the rock.

This explanation is so much the more natural, from the consideration of the circumstance, that the examination of many caverns shows (even without speaking of the fact of "eboulemens" which are so often observed in such situations) the evident marks of different streams of water separated by intervals of dryness, beds of ossiferous gravel alternating occasionally so often as three times with beds of stalagmite (cav. de Schockier, près de Liége). The first portions of mud have in general been introduced with violence and deposited in very curved beds, whose undulating surface has been irregularly covered by mud of more recent origin. Less violent currents subsequently passing over these beds, have sometimes covered them anew with a uniform deposit of the same nature, have sometimes disseminated the bodies lying upon the surface of the previously existing mud, and have accumulated them in the irregular sinuosities of the inferior surface; and sometimes, finally, these currents cutting a horizontal section of all the deposits, have necessarily exposed to view, at apparently the same geological level, bodies belonging to very different epochs and very different beds. These different ages of the gravel and mud in undulating beds, are well exhibited in the greater number of the caverns of the South of France, as those of Bise, of Sommières, &c.; and recent alluvion prevails also in some of them. The irregular surface of the ossiferous mud is a circumstance noticed in other caverns, although it has not been sufficiently remarked. M. Desnoyers has observed it in the most distinct manner in the cavern of Banwell in the Mendip hills in England, and M. Bertrand Geslin has pointed it out in the caves of Adelsberg.

Returning again to the comparison of historical and geological proofs, M. Desnoyers points out that a great number of the caverns of Périgord, of Sarladais, of Quercy, and of Guienne, provinces which formed part of Aquitanian Gaul as it was limited by Augustus, shew evident traces of habitations, and even, agreeably to the narrative of Florus, vestiges of very ancient inclosures. In Perigord these are still known by the name of *Cluseaux*, a word which seems to be derived from the application by Caesar of the term *inclusae* to such caverns. In several as in that of Breingues (dep. du Lot), and that of Combe-Grenant (dep. de la Dordogne) described by M. Delpon and M. Jouannet, many bones of quadrupeds, some of them of extinct species, were found buried, like those of Languedoc, upon the same surface which afterwards received the debris of the human species and of rude manufactures. If we examine the external surface of ancient Aquitania, we find that it is nearly as abundantly covered as Brittany, by monuments of Gallic origin. In the department of Lot alone, in the territory of the ancient

Cadurci, M. Delpon has particularised nearly five-hundred "*delmen*," and a large number of *tumuli*.

But it is not to the Gallic or Gallo-Roman period alone to which we are able to refer the habitation of the caverns of the country now under consideration; for it was a practice necessarily kept up in the manners of a nation exposed to the disorders of the almost perpetual wars in which it was engaged for more than ten centuries, during the successive invasions of the Goths, the Saracens, the Franks, the Normans, and the English. We find indeed, a proof to this effect nearly as authentic as that of Florus, six hundred years after that historian. Eginhard informs us (Annal. de Gestis Car. magni, an. 767.) that the king Pepin, after a protracted contest with the Aquitanians and the Wascons, made himself master of the greater number of the castles, rocks and caverns in which the subjects of Waifre, last Duke of Aquitania, defended themselves: *Castella multa, et petras, atque speluncas, in quibus se hostium manus plurima defendebat, capit.* (D. Bouquet, *Recueil des Hist. de France*, t. v. p. 207.)

We find in the department of Lot, numerous traces of caverns which have been inhabited and fortified at different periods: M. Delpon has described several in his interesting statistical account of that department. This practice of living in subterranean abodes for a time or permanently, has by no means ceased in our provinces, for, upon the banks of the Loire alone, fifteen to twenty thousand families of the departments of Loire et Cher, Indre et Loire, and Maine et Loire, have no other habitation but caverns formed in the hills of tufaceous chalk.

But to confine our attention to the caverns containing human bones which occur in the South of France, and even supposing them to belong to the most distant period of history, the argument which MM. Marcel de Serres and Tournal have believed themselves entitled to draw from the coarseness of the manufactured objects discovered, and one which would assign them an antiquity beyond the historical epoch, does not afford the same conclusion to M. Desnoyers. Observations made with accuracy, and a part of the objects of art discovered by M. T. Teissier in the Cavern of Mialet, near Anduze (a lamp and a small figure of baked clay, and copper bracelets), indicate, almost beyond doubt, the Gallo-Roman period; while the careful examination of the human skulls has demonstrated them to be of the Caucasian race, the original stock of the inhabitants of Europe; deductions which confirm the testimony of Florus, even for the neighbouring provinces of Aquitania; for the present territory of Languedoc formed part of the *Provincia Romana*, whose inhabitants, the "*Volces, tectosages* et *arcicomanes*," had not entirely lost their Celtic usages under the government of the Romans.

As to the other objects found in the various caverns of the departments of Aude, Gard, Lot, Dordogne, &c. and in that of the cave of Paviland in England, such namely, as the fragments of imperfectly baked and kneaded black pottery, battle-axes, and the siliceous heads of arrows; long-shaped bones, such as are employed by savages in the manufacture of arrows and lances; bones cut in the form of combs and forks; shells and teeth of animals, pierced so as to serve as amulets, &c. All these objects are frequently met with in the examinations of *tumuli*, *dolmen*, and *oppida*, monuments which were the places of sepulture, the altars, and the last strongholds of the primitive inhabitants of Gaul, of Great Britain, and Germany. They announce the same degree of civilization which raised these external monuments, and have no connexion with antediluvian manufactures.

In fact, notwithstanding the exaggerated views of the supporters of the opinion of the advanced state of our early civilization, an opinion adopted by many learned men of great merit, it is nevertheless proved by a host of historical proofs, that, even at the period of the conquest of Caesar, the greater number of the tribes of Gaul, which had, up to that time, continued to be independent, were still in the habit of tatooing themselves; of painting their bodies; of sacrificing human victims in their sanctuaries of rude stones; of living in dome-like huts, resembling those of savages, and which were placed in the midst of vast forests, and on the banks of rivers; of making use of arms of stone, &c. There are many of the other usages of these tribes which shew that civilization was but in its infancy, and which correspond with the rude monuments left on their territories, and with the objects of manufacture of which imperfect remains have been found in the caverns.

Further, on examining Gallic monuments, there have often been found associated with those objects of manufacture discovered in caverns containing remains of extinct animals, bones of existing species of domestic and wild animals, and especially of stags, sheep, wild boars, dogs, horses, and oxen, and also marine shells analogous to those found at the present day on the neighbouring coasts. This fact has been noticed in Quercy, and also in several other provinces. We may conceive that these bodies may have been placed under the tombs, and under the Celtic altars, to commemorate the sacrifices offered to the Gallic divinities, and more especially to Hesus, the Mars of the Gauls (Caesar says, *Marti, animalia capta immolant*); or the religious and funeral feasts; or, finally, that they have derived their origin from a superstitious belief common to many nations, and which enjoined the placing of provisions near the dead, to serve in a second life. Is it not very probable, that, in certain caverns which may have been used, either at the same time, or successively, as habitations, as places of sepulture, and, as in the case of the Caves of Mithra and of the Druids, as religious retreats, the bones of the more modern animals, and the marine shells which have been found with the human bones and have been superadded to the ancient fluviatile ossiferous mud, may have had an historical rather than a geological origin?

It is in this new sort of repository of *dolmen* and *tumuli*, a repository, so to speak, of an historical and a monumental character, that we ought to search, with circumspection it is true, but still with more perfect security of the authenticity than in the caves, for the remains of certain contemporaneous animals of the more ancient nations of Gaul. We know that the aurochs, the buffalo or savage bull, and certain species of deer, which lived then in the great forests of Gaul and Germany that have gradually been cleared away, have only been insensibly expelled by the progress of cultivation and civilization; for the urus existed in the Vosges in the time of the early kings of France in the seventh century. The Gauls, who were great hunters, regarded as trophies the skins of these animals, and especially those of the buffalo and the deer, which they offered to Cerunnos, their deity of the chase, or fixed to the gates of their dwellings along with the skulls of their enemies. The horns of the urus were also used as cups on festive occasions.

There is then some chance of finding such objects under Druidical stones or tombs; under those, at least, which existed previous to the period when the combustion of the dead and of victims was introduced, and anterior to the epoch of the invasions of the Franks, the Danes, and the Saxons, all of whom preserved for a long time the Celtic or German manners.

By such researches the antiquary may render great service to the geologist.

M. Desnoyers has already examined attentively, under an analogous point of view, the rich collection of the coins of Gaul preserved at the Royal Library. Upon the most ancient, upon those which were not too evidently a mere imitation of ancient Greek and Roman coins, he found most generally, so far as rude designs admitted of them, representations of animals, and particularly of the wild boar, the horse, the wild bull, and the deer, species which, at that period lived in Gaul, and of which we find the bones under Celtic monuments. Much more rarely he found symbolical or monstrous animals, incorrect representations of birds or of other animals common on Greek coins; but the most characteristic and the most abundant are the quadrupeds, which, history informs us, were natives of Gaul, and those which were employed as domestic animals by the inhabitants.

If we should find under the Celtic monuments, bones of the bear, the rhinoceros, or other extinct species, which, on the other hand, we should also find figured on the coins, it is then that we should have the right to conclude for the contemporaneous existence of these animals and the human species, much rather than from their occurring together in caverns, where so many causes may and must have produced such varied changes. Nothing hitherto brought forward contradicts the zoological and geological results established by Cuvier; and the fruits of the very small number of researches made with this new view, have only presented to us species analogous to living ones. Thus M. Blainville, in an examination of some bones from *tumuli* and *tuguria* of the Gallo-Belgic oppidum (Cité de Limes), in the environs of Dieppe, recognised no extinct species among the six or seven species he examined. The animals found were, the dog, the pig, the deer, the sheep, the ox, &c.

According to these various considerations, and other historical evidence adduced by M. Desnoyers, the human bones of caverns, which themselves are of different ages, appear to him to be of no earlier origin than the period of the Gauls and Celts, and sometimes much more recent; and not more anterior to the last grand catastrophes of the globe than Druidical monuments. The author does not see sufficient proofs for believing that, since the establishment of the human race in Gaul, other species of the larger mammalia than those mentioned in history have become extinct.

The question of the human bones of caverns presents three chief points of view.

Either, These bones were antediluvian, like those of the extinct species of mammiferous animals with which they are found associated (the bear, the hyena, the rhinoceros, &c.); and, in that case, the existence of the human race must, in our country, have preceded those last *upraisings* of mountains which have scattered the diluvian gravel, and the great changes of temperature which seem to have contributed to the destruction of these animals:

Or, These large species of animals can have been destroyed only by gradual and natural causes, operating in historical times, or at least since the invention of arts, and the establishment of the human race in the southern parts of France; and, admitting this view of the question, the Gauls must have hunted the rhinoceros and hyena, as well as the urus, the elk, and the wild boar:

Or, thirdly and lastly, The union, on the same subterranean soils, of these different bodies, was merely the result of various accidental causes, which were not simultaneous, and were distinct from the general phenomena of bone-caverns.

Many geologists have declared decidedly for the two first opinions, and for the contemporaneous existence of man and a large number of the species of animals which have totally disappeared. The opinion advocated by M. Desnoyers is directly the contrary, and is that which we have mentioned third in order. This opinion, which appears to have become that of the largest number of geologists, does not in any degree diminish the interest of the discoveries from which MM. Marcel de Serres, Tournal, de Christol, Farines, &c. have drawn opposite conclusions, and have reproduced the idea of the existence of fossil remains of man, a view contradicted by the phenomena of all other deposits. It is possible, that, when they shall have regarded the question in a mixed geological and historical point of view, it may seem to them more doubtful, and that they will require to illustrate it by new observations.

One thing, however, is certain, viz. that the fact of the occurrence of human bones and objects of industry in caverns, is not new; for it had been previously pointed out in the caverns of Franconia, by MM. Rossenmüller and Sömmering; and in those of England by Mr Buckland, who, indeed, has made it the subject of a separate chapter in his Reliquiæ Diluvianæ; but not one of these learned men has, any more than Cuvier, concluded, that man and the large species of extinct animals were contemporaneous,—that is, regarded them as antediluvian; so much has the loose and gravelly soil at the bottom of these caverns been exposed to alterations posterior to the original deposit, and caused by the action of repeated currents of water.

ADDITIONAL OBSERVATIONS ON THE STRUCTURE OF RECENT AND FOSSIL CONIFERÆ.

By WILLIAM NICOL, *Esq., Lecturer on Natural Philosophy. With a Plate. Communicated by the Author.*

SINCE the publication of my paper on the structure of Recent and Fossil Coniferæ, in the last Number of the Edinburgh New Philosophical Journal, I have received small pieces of the Araucaria excelsa, of the Dammara australis, of a species of Callitris, and of the Pinus lanceolata, and shall now subjoin a short notice on the structure of each.

First, then, with regard to the *Araucaria excelsa*, or Norfolk Island pine, it is to be observed that it differs from the araucaria from Moreton Bay in New Holland, in having regular annual layers. These, however, have not the distinct linear boundaries observable in those of the pines of cold and temperate regions. They are indicated merely by one or two concentric rows of meshes, a little smaller than those occurring either without or within, and by their partitions being a shade darker than the rest.

The first figure of Plate V. represents a small portion of a transverse section, chiefly with a view to shew the state of the boundaries of the annual layers. It was taken from a part of the section in which the reticulated texture had the most regular form, for, as in the Moreton Bay species, the meshes assume very different forms in different parts.

In the longitudinal section parallel to a radius, the discs or areolæ of the Araucaria excelsa are similar in every respect to those of the Moreton Bay species; but whilst in the latter, as far as my observations have extended, they most frequently occur in single rows, in the former they most frequently occur in double rows. In the excelsa, triple rows are often to be seen; and in these as well as in the double rows, the polygonal portions of the discs are well defined. In many of the triple rows, the discs of the middle row have the most perfectly regular hexagonal form. The side-rows have their polygonal parts equally well defined and rectilinear; and in both the triple and double rows, the contiguous sides of the discs are connected with each other, by two fine lines or fibres, at or near each of the angles. To see these connecting lines distinctly, a very strong light is required. The form of the discs, both in the double and triple rows, intirely depends on the distance of the discs from each other. When the discs are at a certain distance, the form is polygonal; but when the distance is a little greater, the form is circular.

When a section has been cut of the proper thickness, the discs display at the circumference two very distinct concentric curves when the form is circular, and two distinct lines on each of the straight sides when the form is polygonal. At the centre there is always a circular opening, and surrounding this several concentric curves may often be seen, whether the form be circular or polygonal. The discs often appear of a pretty uniform brownish-red colour, without shewing any concentric curves near the centre, or double lines at the circumference, depending apparently on the thickness of the slice. When the slice is very thin, many of the discs display no colour at all, and merely present two curves, one at the centre and another at the circumference. A whole row of discs, too, is sometimes to be seen lying in a plane very much inclined to that of the section containing it. In such cases the discs present their edges to the eye with a slight degree of obliquity, and shew that their thickness bears a very small proportion to their diameters.

The discs, whether in single, double, or triple rows, are always placed near one another, and never occur dispersed at different distances, which they frequently do in most of the true pines. The number of discs in a row, too, is extremely various. Sometimes there are not more than one, two, three, or four, and sometimes there are as many as sixty, seventy, or even eighty, but in general they are not nearly so numerous.

The second figure in Plate V. represents a portion of a longitudinal section parallel to a radius of the Araucaria excelsa. In the upper part there are both double and triple rows of discs, and in parts of the figure there are no discs, a circumstance which occurs in every species of the coniferæ. The under part of the figure represents one of the most elegant groups of discs in double rows that I have ever yet observed. It occurs in the same section from which the upper part was taken, but at a little distance from it, and this is the reason why the figure is separated into two distinct portions. The figure is magnified in the same degree as those in Plates II, III, and IV, in the last number of the Journal, namely, about four hundred times. There was not room in the plate for a concentric longitudinal section; and indeed had it been introduced, it would have shewn nothing different from the one formerly given of the Araucaria from Moreton Bay.

The *Dammara australis*, however it may differ in its botanical characters, resembles so closely in the internal structure of its wood the Araucaria excelsa, that the one cannot be distinguished from the other. Both have regular annual or periodical layers, and in both the layers are similarly terminated. The form of the meshes are also similar. The discs or areolæ in the longitudinal section parallel to the radius, are similar in size, form, and arrangement, and in the longitudinal concentric section, the expansions in the partitions are equally similar.

It is worthy of remark, however, that the cohesion of the radial partitions in the transverse section of both these species, is much greater than in the Araucaria from Moreton Bay; and that in this respect the Dammara is somewhat superior to the Araucaria excelsa.

The species of Callitris I have now to notice, is a new one from Morton Bay, and is known in New Holland by the name Moreton Bay cedar. It shows no indication of regular annual layers, but there are indications of the process of vegetation having sustained slight and very irregular interruptions. On cutting a thin transverse section, the slender cohesion of the radial partitions is equally striking as in the Moreton Bay Araucaria, it being equally difficult to cut one, even of small extent, without these partitions separating from each other. The meshes of the transverse section are also very irregular in respect of form. But whilst in these particulars the two kinds of wood agree, they differ most widely, as seen in the longitudinal section parallel to a radius. In the Callitris, the discs which occur both in single and double rows, resemble those occurring in many of the true pines. In the double rows they are placed side by side as in the pines, and whether in single or double rows they are similar in size as well as in form.

The Pinus lanceolata (Cunninghamia lanceolata of Brown) so greatly resembles, in its general aspect, as well as in the form of its leaves, the tribe of Araucaria, that I was led to expect that its internal structure would be somewhat similar. On cutting the requisite sections, however, a very different result was obtained. The transverse section presents well defined annual layers. The quadrangular form of the meshes predominates, and, as in many of the true pines, the meshes gradually diminish in size towards the outer limit of each annual layer. The longitudinal section parallel to a radius, has discs similar to those of the pines, and the concentric longitudinal section has expansions in the partitions equally similar.

In my former communication on this subject I stated, and that, too, entirely from the anatomical structure of the wood, that the *Salisburia adiantifolia* belonged to the coniferæ. I have since been informed that botanists at present maintain that it does not. At a former period it was classed with the coniferæ, and it is not impossible that, at some future period, some one may again restore it to its former place. At all events there can be no dispute as to the structure of its wood being similar to that of the trees which botanists have styled coniferæ; and that those who have not had an opportunity of examining the wood may be enabled to form some ideas of its structure, there is introduced into Plate V. representations of the three usual sections. Figure 3 is transverse, 4 is longitudinal parallel to a radius, and 5 is longitudinal concentric.

In conclusion I may be permitted to observe, that the existence of regular hexagonal discs in recent coniferæ, shews the impropriety of constituting a separate genus for those fossil species having discs of the same form, and arranged in the same manner. In the true pines, in the junipers, thuyæ, and cypresses, the discs are always circular, but of very different dimensions; and wherever they are arranged in double rows, those in the opposite rows are always placed side by side, and a similar form and arrangement is often to be seen in fossil species. But in the tribe of araucarias, the discs are sometimes circular and sometimes polygonal: and wherever they are arranged in double or triple rows, those in one row always alternate with those in the adjoining row or rows; and a similar form and arrangement is sometimes to be seen in fossil species, as that at Craigleith.

CHARACTERS OF THREE GENERA OF INDIAN PLANTS. *By G. A. WALKER ARNOTT, Esq. A. M., F. L S., &c.*

1. NATSIATUM, *Ham.*

DIŒCIA PENTANDRIA.

Ord. Nat. PHYTOCRENEÆ, *Arn.*

Flores diœci. *Calyx* 5-partitus; laciniis lanceolatis, persistentibus. *Corolla* hypogyna, alte 5-partita; laciniis lanceolatis, acuminatis, recurvis, lobis calycinis alternantibus, iisdem longioribus. *Torus* brevis, cupuliformis, fere liberus, 5-lobatus; lobis laciniis calycinis oppositis, bidentatis; dentibus linearibus, imo apice obtusis ac incrassatis. MASC. *Stamina* 5, petalis alternantia, circa pistilli rudimentum hirsutum ad basin corollæ inserta. *Filamenta* perbrevia. *Antheræ* erectæ, ovatæ, apiculo brevi terminatæ, 2-loculares, longitudinaliter dehiscentes. FŒM. *Stamina* 5 abortiva. *Ovarium* liberum, globosum, hispide pilosum, uniloculare, 2-ovulatum. *Ovula* ex apice loculi pendula. *Stylus* subnullus. *Stigmata* duo, elongata, recurva, papillosa.

Volubilis. Folia *alterna, cordata, repando-dentata, scabriuscula.* Racemi *axillares, elongati, hirsuti, penduli.* Flores *parvi, viridescentes.*

1. N. *herpeticum*, Ham.—*Wall. List of E. I. Plants*, N. 4352.
HAB. Bagduyar. *D. Hamilton.*

Hamilton, in his Herbarium, refers this to the Rhamneæ, with which it has no affinity whatever. It has so very many points in common with Phytocrene of Wallich, that I do not think there can be any doubt of their together constituting a small order, bordering on the one side on Menispermaceæ, on the other on Urticeæ.

2. BHESA, *Ham.*

PENTANDRIA DIGYNIA.

Ord. Nat. CELASTRINEÆ, *Brown.*

Calyx 5-fidus; segmentis recurvis. *Petala* 5, calycis lobis alternantia, sub margine tori inserta, recurva. *Torus* discoideus, carnosus, tubo calycis arcte adnatus, 5-lobatus. *Stamina* 5, petalis torique lobis alternantia, margine tori inserta. *Ovarium* sessile vel basi toro paullulum immersum, globosum, inferne glabrum, apice lanugine obtectum, 2-loculare. *Ovula* ad basin cujusque loculi duo, erecta. *Styli* duo, filiformes, distantes, flexuosi. *Stigmata* parva, capitata.

Frutices vel *arbores.* Folia *exstipulata, alterna, glabra, petiolata, elliptico-oblonga, integerrima, penninervia, venis transversis striæformibus creberrimis pulchre notata.* Flores *spicati, parvi.*

1. B. *Moja*, Ham.—Spicis axillaribus, simplicibus, *Herb. Ham.* N. 788.

2. B. *paniculata*, Arn.; spicis ramosis, paniculam terminalem efformantibus.

Hab. In Insula "Prince of Wales Island," dicta.

The first of these species I have only seen in Hamilton's Herbarium, now belonging to the University of Edinburgh: corresponding specimens were deposited at the India House Museum; but I have not been able to discover any reference to it in Dr Wallich's List. The other I received from my friend Dr Hooker. At first I felt inclined to consider this genus as the same with *Moacurra*, of which, although specimens have been distributed by Dr Wallich (*Celastrus acuminata*, Wall. List N. 4342, pp. 152, 250), I have not been so fortunate as see any, and therefore must trust to the accuracy of Roxburgh's description, from which it appears that in it the disk or torus is different from that of *Bhesa*, and the ovules are pendulous from the apex of each cell.

3. SCHUMACHERIA, *Vahl* (non *Spreng.*)

MONADELPHIA POLYANDRIA.

Ord. Nat. DILLENIACEÆ, *DC.*

(Charactere reformato.)

Sepala 5, persistentia, extus intusque adpresse sericeo-villosa; 2 exteriora, 3 interiora, æstivatio imbricativa. *Petala* 5, decidua, hypogyna, calycem subæquantia, sepalis alternantia; 2 margine crispula: æstivatio imbricativa. *Stamina* hypogyna, numerosa, unilateralia, pluri-serialia, monadelpha, omnia fertilia. *Filamenta* brevia, basi in anthophorum crassiusculum complanatum coalita. *Antheræ* lineares, elongatæ, biloculares; connectivum in mucronulum productum. *Torus* nullus. *Ovaria* 3, libera, villosa, 1-locularia, 1-styla, 1-ovulata. *Ovulum* e basi loculi adscendens, arillatum. *Styli* simplices, terminales, filiformes, glabri, per æstivationem staminibus incumbentes. *Stigmata* simplicia.

Frutex *ramosus.* Rami *glabri, purpurascentes, teretes; novelli complanati.* Folia *alterna, coriacea, glabra, lævia, petiolata, exstipulata, repando-serrata, serraturis mucronatis, penninervia, subcoriacea; nervis parallelis, simplicibus.* Petiolus *canaliculatus, basi dilatatus, semiamplexicaulis.* Spicæ *paniculatæ terminales et e foliorum axillis superiorum, folia subæquantes.* Flores *sessiles, secundi, basi bibracteati.*

1. S. *castaneifolia,* Vahl.; α; *Vahlii,* "foliis parum attenuatis, basi apice acutis."—S. castaneifolia, *Vahl in Skrift. Naturh. Selsk. Kiobenh.* 6. p. 122.—β. *Grahamii,* foliis basi apice obtusiusculis.—Pleurodesmia Grahamii, *Arn. mss.*

HAB. Ceylon.

My specimen, from Dr Graham, has the leaves shorter and more obtuse than Vahl attributes to his plant; but I cannot look on them as distinct species. I have also altered somewhat the character given by Vahl: thus Vahl mentions only three petals, one of which is larger than the others, and curled; and one ovary, with three styles; but all the other points agree so well, that now I have no doubt but two of the petals had fallen off before he examined the flower, and that he was deceived into a belief of there being only one ovary, by the thick coating of white hairs, which at first sight obscures the true structure. It is, as a genus, most allied in habit to *Delima,* but is easily distinguished by the structure of the stamens: in character it approaches to *Pleurandra,* particularly to the section called *Candolleanæ,* but differs in several particulars.

ARLARY, 2*d December* 1833.

ON THE STRUCTURE AND HABITS OF THE *LIMNORIA TEREBRANS,* A MINUTE CRUSTACEOUS ANIMAL, DESTRUCTIVE TO MARINE WOODEN ERECTIONS, AS PIERS, &c.

By JOHN COLDSTREAM, *M. D. M. W. S., &c. Communicated by the Author*[*].

History.

THE minute crustaceous animal which is the subject of it, claims our attention, both on account of the peculiarity of its habits, and its agency in the destruction of some valuable works of public utility. It inhabits the ocean and its shores, dwelling in holes in wood, which it forms for itself by burrowing. As it is gregarious, and very abundant in situations favourable for the exercise of its habits, it soon produces great effects on the wood to which it attaches itself. By boring in all directions, it so disintegrates it, as to allow the sea to wash away its surface: and thus, layer after layer of the wood is first riddled by the borer, and then abraded by the sea, until the whole piece of timber attacked is completely destroyed.

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[*] The description here given may be considered as to a certain extent supplementary to that published by Dr Leach, in the Linnean Transactions, vol. xi. p. 370.

The ravages of the limnoria were first particularly observed by our distinguished civil-engineer, Mr Stevenson, who, while engaged in the erection of the Bell-Rock Lighthouse, noticed that the beams of wood, used in some of the temporary works, were destroyed by its borings in a very rapid manner. He sent specimens of the animal, contained in the wood which it had bored, to Dr Leach, who determined it to possess characters sufficient to entitle it to be considered as the type of a new genus, to which he assigned the name of *Limnoria*, placing it in the family Asselides, of the order Myriapoda, in his new arrangement of the Crustacea. In a systematic enumeration of the same class of animals, afterwards published by Dr Leach, he placed the Limnoria in the order Isopoda,—subclass Edriophthalmia,—of the class Malacostraca, which is characterised by sessile eyes, palpigerous mandibles, and the want of appendages to the legs. The genera grouped in this arrangement along with the Limnoria are Eurydice and Cymothoa. Latreille and Lamarck make the Limnoria only a species of the last named genus. It is chiefly composed of animals, which are the parasites of whales and fishes, sucking their blood, and differing in other respects so much from the Limnoria, although they resemble it in general structure, as to justify their being dissociated from it.

Since it was first observed at the Bell-Rock, the Limnoria has been found on many parts of the coasts of Britain, and has, in some places, been the cause of most serious injury to piles of wood supporting various useful erections on the shores of the sea. However, notwithstanding the extent and importance of its ravages, the animal has not yet met with that attention from naturalists which has been bestowed on other agents of destruction affecting the interests of man. Dr Leach announced it, in

1811, as a "new and highly interesting species," sent to him "through the politeness of his attentive and worthy friend, Robert Stevenson, Esq.," and stated, that "it occurs in the greatest abundance at the Bell-Rock, in the old wood-work, used whilst the light-house was building, which it perforates in a most alarming manner, entering to the depth of two inches or more, boring in every direction." But, although the characters and general description of the animal given by Dr Leach have been copied by many systematic writers, I am not aware of any other naturalist having given to the public the results of extended inquiries concerning it. I shall therefore state all the facts relating to its structure and habits, which I have succeeded in ascertaining satisfactorily, being convinced that we ought to know, as well as possible, every thing connected with the natural history of an animal, which, although so minute, exerts so powerful an agency with regard to us. But I am far from supposing that I have exhausted the subject. There are many points in its structure which I cannot make out satisfactorily, and I have wanted opportunities of ascertaining what are its habits in certain circumstances, and the modes in which it exercises some of its functions.

I. *External Characters.*

The average length is $\frac{15}{100}$ths of an inch, and the breadth $\frac{4}{100}$ths; but several individuals occur $\frac{2}{10}$ths of an inch in length, and of proportionate breadth. The female is about one-third larger than the male. The usual colour of the dorsal surface is a pale greyish-brown; some individuals are streaked about the head with lines of a darker shade. The legs and ventral surface of the body are almost white. The body is in general translucent, but not so much so as to admit of the internal organs being seen distinctly. The form is represented in Figs. 1. and 2. (Pl. VI.) It is semicylindric, convex above, slightly concave beneath, rounded before and behind. The breadth being considerably less than the length, the general aspect is somewhat vermiform.

The whole animal is composed of fourteen segments, of which one forms the head, the seven next to it bear each a pair of legs, and the remaining six constitute the tail. The head is rather narrower than the other segments; the fifth is the broadest, the second, third, fourth, and fifth, are nearly equal in length. The succeeding six are considerably shorter, while the two forming the last divisions of the tail are the longest of the whole. The head is nearly spherical in form, and bears four antennæ, two eyes, and the organs of the mouth. Each of the seven succeeding segments is provided with a pair of articulated feet, and four of them with scales on the ventral surface, forming a kind of sac, in which the young are carried for some time after their exclusion from the egg. The segments of the tail bear, ventrally, six pairs of scale-like appendages, arranged in three rows, and two pairs of styles, one pair projecting on either side from under the terminal edge of the last segment. The whole of the dorsal surface is covered with stiff pinnated hairs, longer than those of other small crustacea. Amongst these hairs numerous minute parasites, somewhat like Echinorynchi and Acari, are constantly to be seen. They even infest the branchiæ.

II. *Anatomical Details.*

In so far as the structure of the Limnoria agrees with that of other and well-known crustaceous animals, I shall not describe it minutely; the chief object of such memoirs as the present being, as I conceive, to point out those peculiarities which distinguish a particular species from its congeners, and fit it for the purposes it is destined to fulfil. With regard, therefore, to the organs of support,—the frame-work of the organization which is to engage our attention,—I need only remark, that, in their constitution and arrangement, they are the same as those of allied animals. The crust covering the segments of the body and of the tail is rather soft, but distinctly calcareous. It is harder, and even brittle on the head. It effervesces with acids, but not strongly. It is translucent in the fresh state, so as to admit of the motions of the legs being seen through the body, as well as the general arrangement of the viscera. The ligaments connecting the segments are somewhat more brittle than those in similar animals.

The organs of motion are the fourteen legs, the tail, and certain subcaudal appendages. These last are used in swimming, but ought to be regarded chiefly as organs of respiration.

A pair of legs is attached to each of the seven segments of the body. They are articulated just under the sloping edge of the dorsal crust, and, being partly folded beneath the abdomen, a small portion only of each is seen to project laterally when the animal is in motion. Their general arrangement is represented in fig. 2. (Pl. VI.), and the details of their structure in figs. 3, 4, and 5. They are composed of from four to six articulations. The longest are the first and fifth pairs. Those of the first are represented in fig. 3; the whole length is about $\frac{5}{100}$ths of an inch, and there are four articulations. The first articulation is the longest. The second bears upon its inferior surface a row of tubercles. The claw is strong and sharp. The legs of the second segment are shorter and thicker than those of the first, and have the tubercles less fully developed. Those of the third and fourth are similar to these. (Fig. 4. Pl. VI.) That of the fifth (fig. 5.) is more slender, composed of six articulations, of which the two first are of equal length. The sixth and seventh pairs of legs are similar to it, but somewhat smaller. The terminating claw is, in all, single, and somewhat hooked. All the legs bear short hairs scattered over their surface.

I have no details to offer regarding the muscular or the nervous systems. The extreme minuteness of the parts renders it almost impossible to examine them satisfactorily, even with the aid of high magnifying powers. It is not improbable that the nervous system bears some analogy to that of the Cymothoa, which, being a much larger animal, has been successfully anatomized by Messrs Audouin and Millne Edwards. (Annales des Sciences Naturelles, tom. xiv. p. 77.) From their researches it appears, that the Cymothoa has one large ganglion in each segment, which sends off two nervous filaments on either side to supply the viscera and legs; and two filaments of communication, anterior and posterior, connecting it with the ganglia of the other segments. These last lie close together, so as almost to form one cord.

The only organs of sensation discoverable in the Limnoria, are the four feelers, two eyes, and the compound styles which project from the caudal segment. The feelers, or antennæ, are placed on the most prominent part of the head, a pair on either side, one of each above the other. The superior ones arise very close to one another, near the median line; the inferior are more widely separated, and therefore more external, than the others. Each superior antenna (Fig. 6.) is composed of four articulations, and bears at its point several long bristles. The lower ones (Fig. 7.) have five articulations, and only a few short bristles. They are somewhat longer than the others. The eyes are placed on the sides of the head, a little anterior to the extremities of its longest transverse diameter. Viewed with low magnifying powers, they appear to be only simple, circular, convex, corneæ, through which a deep black colour is transmitted; but, with high powers, they are seen to be compound eyes; each cornea being formed of eight circular portions, seven set round the circumference, and one in the centre. They project very little beyond the general level of the contiguous surface. The deep black colour, which is remarkable in the eye even of the fœtus, appears to be owing to a choroid coat common to all the corneæ. The position and external appearance of the eye are represented in Fig. 8. It is uncertain whether we ought to regard the caudal styles as organs of sensation. But their situation and structure make it not improbable that they are used as feelers *. They are attached to the ventral surface of the terminal edge of the last segment. There are two styles on either side, arising from a common peduncle. One, which projects laterally, is short, and composed of three or four articulations; the other projects behind, has two articulations, and several long bristles. (Fig. 1.)

The organs of digestion, which have been satisfactorily made out and traced, are, two pairs of jaws, a pair of strong mandibles, four tubiform organs connected with the œsophagus, a stomach, and intestine. The general arrangement of the jaws and mandibles, and their connexion with the head, will be best understood by reference to Figs. 2 and 8. The two pairs of jaws are represented in connexion in Fig. 9, and the inner pair detached in Fig. 10. The jaws are articulated with the corselet at its inferior and posterior edge, and their muscles arise within the head. The external jaws have each two articulations, one of which is placed laterally, and does not enter into

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* The correctness of this view, may, perhaps, be doubted when it is considered, that, in some onisci, a thick fluid has been seen issuing from the caudal styles.

the formation of the mouth; the other is narrow near its base, and increases in breadth anteriorly; its inner edge is straight and inflected, so that the surface which bounds the oral fissure is broad. It, as well as the other parts of the jaw, is covered with short stiff hairs. The internal jaws (Fig. 10) are of more simple structure. They are of a lengthened quadrangular form. Their free extremities are tipped with bristles. The mandibles are placed obliquely, and in such a manner as that their sharp points project from the most advanced part of the head when the jaws are moved aside. Their form is shewn in Fig. 11, and their attachment to the corselet in Fig. 12, where *a a* point out the situation of the mandibles. Each is provided with two very sharp and hard points, coloured brown, one of which is in contact with its fellow of the opposite side, the other projects anteriorly. The mandibles are about $\frac{1}{80}$th of an inch in length; their points are perfectly smooth; they are furnished with palpi, one of three articulations being attached to each. These are represented in Fig. 11.[*] The mandibles are doubtless the boring organs; and it is easy to conceive how powerful they must be, notwithstanding their extreme minuteness. They have, however, fewer peculiarities of form and structure than one might expect to find in an animal whose habits are so dissimilar to those of other crustacea. Here, as in many other cases, we see that, by a very small alteration in structure, the organ is adapted to a purpose differing much from its usual appropriation in other animals. It is not by creating new organs that a new function is fulfilled, but very often merely by changing, and that but slightly, the forms of organs already in existence. In some of my examinations of the oral organs, I have indistinctly seen what seemed to be funnel-shaped lips within the mandibles, fitted for protrusion beyond the jaws, and apparently for suction. Of this, however, I am uncertain; nor can I describe particularly the course or the form of the œsophagus; but, connected apparently with that organ within the head, and hanging down in the cavity of the body as low as the fourth or

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[*] M. Desmarest, in his valuable systematic treatise on crustaceous animals (1825), places the limnoria in the order Isopoda of the class Edriophthalmia, and gives, as one of the characters of that order, that the species included in it have no mandibular palpi. I am satisfied that they do exist in the limnoria.

fifth segment, are distinctly seen four tubiform tapering organs, closed at their distal extremities (see Fig. 12, *bb.*) These vary in length in different individuals, but, generally, there are two longer and two shorter. They seem to be filled with a tenacious transparent substance, intermixed with very minute irregularly-shaped particles; sometimes they are annulated. Organs nearly similar are found in the common slater (*Cloportus asellus*), but in it, instead of being straight, they are waved. They appear to bear some analogy, in situation and structure, to what have been termed salivary or hepatic vessels in some coleopterous insects, particularly in *Lixus angustatus* and *Coccinella argus.* (Dufour, Recherches anatomiques sur plusieurs Insectes Coléoptères. Ann. des Sc. Nat. tome iv. 103.)

The œsophagus seems to bend upwards after leaving the head, and to terminate in the stomach, about the middle of the first segment of the body. There the alimentary canal is first distinctly visible through the dorsal crust. The transverse diameter of the stomach is equal to about $\frac{1}{8}$th of the breadth of the body; it is generally seen filled with yellowish-white matter. From the middle of the first segment, the stomach descends obliquely towards the ventral aspect, till it reaches the sixth segment, where the canal is suddenly recurved upon itself upwards, and becomes contracted, marking, probably, the commencement of the intestine, which is seen again expanded about the middle of the fifth segment (see Fig. 1, where the dark markings in the dorsal aspect represent the alimentary canal, so far as it is generally visible through the crust; and Fig. 12, which is intended as a diagram of the whole alimentary canal, the two pairs of jaws having been removed.) The intestine is quite straight, and terminates beneath the posterior margin of the last segment. It is generally stuffed with yellowish pulpy matter.

The organs of circulation have hitherto eluded my search. Although aided by high magnifying powers, I have never seen the globules of the blood in the Limnoria carried along in the torrent of the circulation, as may be very satisfactorily seen in some other minute crustacea; nor can I detect any dorsal vessel. But the respiratory organs are less obscure. They consist of six pairs of scale-like bodies, pendant from the anterior segments of the tail. There are three pairs of an irregularly oval form (Fig. 14), and three pairs nearly quadrangular (Fig. 15), both sets being edged with long and pinnated bristles. The oval ones are marked with lines radiating from their centres, and, between these, there are seen numerous spherical bodies (see Fig. 14.) These branchæ are arranged in three rows, in an imbricated manner, one of each kind being articulated together on a common peduncle on either side; (see Fig. 13, which represents one of the caudal segments, with two pairs of the branchial lamellæ hanging from it.) The oval ones are situated externally with regard to the others. The branchial scales have no general covering, such as exists in several other animals of the same class.

I shall allude here to an organ situated within the cavity of the tail, behind the branchiæ, the function of which I am ignorant of. It consists of two vesicular bodies, of an oval form, attached to a common peduncle (see Fig. 16.) I have always seen these bodies filled with numerous minute, dark-coloured granules, of an irregularly round shape, which could not be detached from the substance in which they were imbedded, by tearing it up. They exist in both males and females.

The chief facts relating to the reproductive system which have been well ascertained, are, 1*st*, That the ova, during at least a certain period of their existence, swim in a fluid contained in the general cavity of the body, (perhaps enclosed within thin sacs, lying along either side of the cavity, throughout its whole length, and forming ovaries); and, 2*dly*, that the female carries its young in a pouch formed of scales, pendant from the third, fourth, fifth, and sixth segments. With regard to the first fact, it may be doubted whether the bodies alluded to as being diffused through the abdominal cavity, be truly ova; but it is difficult to conjecture what else they can be. In some individuals, when a wound is made in the ligament connecting any two of the segments, a gush of fluid instantly takes place; and on examining this with the microscope, there are seen in it innumerable transparent spherical bodies, varying from $\frac{1}{300}$th to $\frac{1}{4000}$th of an inch in diameter. No trace of internal organization is discoverable in these. The small globules exactly resemble the larger ones in every thing except size. They remind me of the globules which are found within the abdominal cavity of many marine *Annelida*, and which vary from $\frac{1}{90}$th to $\frac{1}{500}$th of an inch in diameter. Of the oviducts, and the arrangement of the ova before their maturation, I know nothing; but I have frequently found, both in autumn and in spring, the fœtus within the abdominal pouch, which I have already mentioned as existing in the female. This pouch is formed of eight or ten scales, hanging in pairs from three or four segments between the third and the seventh. Their general disposition is shewn in Fig. 2; their form is ovate; their edges are united, probably by an internal membrane lining the sac. Within the sac thus formed, there are generally found six or seven young limnoriæ, in some individuals only five, in others nine. They have come under my observation always in an advanced stage of development, but I have never seen them give any signs of life. The head and the other five anterior segments are larger proportionally than in the adult. The antennæ and eyes are almost completely formed, although the articulations of the former are not distinctly seen. The colour of the eye is nearly as deep as in the adult; the other appendages hang loosely from the inferior surface: all present the appearance of simple tubiform organs. Even the jaws and the branchiæ can scarcely be distinguished from the legs. There are representations of the dorsal and lateral aspects of the fœtus given in Figs. 17 and 18.

III.—Performance of Functions and Habits.

Observations are awanting to shew to what extent the Limnoria enjoys, and in what mode it exercises, the functions of sensation. When any part of its body is touched rudely, it rolls itself up almost into a spherical form; approximating for a few seconds the head and the tail, by incurving the inferior surface, in the same manner as is done in similar circumstances by several animals, both terrestrial and marine, which resemble it in structure; particularly by Sphæroma serrata, Typhis ovoides, and the medicinal wood-louse, Armadillo vulgaris.

While creeping, the Limnoria moves its inferior antennæ in all directions, and seems to be very sensible to the least impression made upon them; but the superior pair are generally folded back upon the head above the eyes.

Locomotion is performed by creeping and swimming. It creeps very slowly, backwards as well as forwards, and seems to drag itself along. But it swims rapidly, using chiefly the caudal lamellæ, which it moves in the manner of oars. Sometimes also it propels itself, by a sudden movement of its tail, backwards, to the distance of an inch or two, through the water. When taken out of the water, and laid upon its back, it cannot right itself. It often swims in circles, with the head directed downwards, and sometimes it narrows these circles so much that it seems to wheel round upon its head as a centre.

In considering the digestive function, we are naturally led to inquire, 1. What is the nature of the animal's food? 2. How is the food procured? and, 3. How is it prepared for being received into the internal digestive tube? With regard to the first point,—the nature, namely, of the Limnoria's food,—it appears to me to be sufficiently established, that the animal feeds upon the wood through which it bores, and that from the following circumstances: *First*, That no woody particles are seen to float out from the hole when the animal is engaged in boring under the eye of an observer, the side of the hole being cut away to obtain the view: *Secondly*, That I have never seen it attack any other substance with its mandibles. *Thirdly*, That the contents of the stomach resemble comminuted wood.

It would seem, therefore, that the Limnoria furnishes an exception to the general proposition, stated by many naturalists, —that all the crustacea feed on animal substances.

Many kinds of wood are devoured by the Limnoria. On this coast, it is generally in fir-timbers that it is found. But at the Bell-Rock, Mr Stevenson saw it attack oak, black birch, and other woods; and indeed, teak was the only wood exposed to its ravages that was not perforated. I am not aware of any fact proving that the Limnoria attacks floating timber. It seems to attach itself in preference to wood fixed on the sea-shore. And it is of importance to remark, that it often effects a lodgement in piles, very near high water-mark, where it is left dry by the receding tide during the greater part of the twenty-four hours. I see no reason, however, why it should not attack floating timber, not covered by something impervious to its mandibles. Our coasting vessels are generally sufficiently protected from its attacks by frequent pitching; and other vessels by copper-sheathing.

The second point of inquiry is, How is the food procured? and this leads us to the examination of the peculiar process by which the limnoria is distinguished, namely, its *boring*. In commencing its ravages on an entire piece of wood, it would seem that the limnoria fixes first upon the soft parts, situated between the harder annual layers. After effecting for itself a lodgement within the wood, by excavating a hole somewhat larger than is sufficient to contain its body, it directs its workings upwards, at an angle of about 45°, and keeps, in preference, in the course of the soft layer into which it bored at first. In this process, the mandibles seem to be the most effective tools.

When a section is made of a piece of wood, filled with limnoria, recently taken from the sea, and the wood is immersed in salt-water in a glass vessel, of a form convenient for its close examination, if care shall have been taken to cut, so as to expose some of the holes just at their internal extremities, the animal may occasionally be detected in the act of boring. It is seen to apply the mouth to the surface of the wood, and slowly to rotate its body by the aid of its legs, placed against the walls of its gallery. I have found it impossible to perceive the action of the jaws or mandibles during the operation. It seems to be necessary to its working that the hole should be filled with salt-water. It has been already stated that the animal bores upwards; but very often its galleries are horizontal, and sometimes perpendicular, either upwards or downwards, but chiefly in the former direction. Their course is, for the most part, tortuous. They are cylindrical, or very nearly so. Their diameter varies from $\frac{1}{20}$th to $\frac{1}{15}$th of an inch, and is nearly the same throughout the whole length of the hole; which would seem to indicate that, as the animal increases in size, it leaves its old workings and begins new ones. Their walls are as smooth as the surface of a piece of wood of the same kind, which has been cut by a sharp knife. They have no visible living of any kind. Their terminations are seldom more than two inches from the surface of the wood. The hard ligneous layers of the circles of annual growth, are much less generally perforated than the softer layers; so that, in a piece of timber much destroyed, (such as is figured in fig. 19, the dotted lines surrounding which, mark the original size of the pile represented), these, as well as the nuclei of the branches, stand out in relief, dissected, as it were, out of the softer materials which had surrounded them. Although many workings approach very near one another at the same depth from the surface, yet they preserve their original direction so well, that they seldom or never open into one another.

Such are the results and consequences of the limnoria's appetite for the woody fibre; and having, by the means, and with the circumstances above described, procured its natural food, we may imagine, although we cannot see, how it uses its strong mandibles in the comminution of the particles of the wood, so as to fit them for being submitted to the digestive powers of the stomach.

Of the subsequent processes of chylification and assimilation, nothing is known. And I have already stated that the circulation has hitherto escaped my notice. There remains for me, therefore, under this head, only to add the few facts relating to the function of respiration which I have collected.

When the animal is at rest in sea-water, the branchiæ are constantly fluttering with a slow motion, being alternately moved out from the ventral surface, and then closely adpressed against it, twenty or thirty times in a minute. The current produced in this way is about equal to the length of the body. When any particles of dust clog the branchiæ, the posterior pair of legs is used to disencumber them. In swimming, the branchiæ are moved with much greater rapidity, also when the animal is placed in fresh water. The current is then considerably longer. The limnoria continues to live for several hours in fresh water. When the motions of the branchiæ become languid, the internal layer only is put in motion. On being taken out of the water, it moves with extreme slowness, but lives for many hours. Its branchial lamellæ remain motionless.

IV. History of the Ravages committed by the Limnoria.

The circumstances connected with the erection of the Bell-Rock Lighthouse are so well known, that it is unnecessary for me to enter into particulars regarding the position and arrangement of the timbers supporting the temporary wooden beacon, in which the intrepid engineer and his assistants resided during a great part of the time that they were engaged in their hazardous labours. I shall only state, that the temporary beacon was supported by twelve large beams of Memel-fir, fixed in batt-holes cut for them in the rock ; the sides of which beams were charred and pitched ; but their soles, being closely opposed to the rock, it was thought unnecessary to protect in a similar manner.

In the course of the second summer's operations, that is in 1808, it was found that the edges of these timbers were in a state of decay, and on examining into the cause, Mr Stevenson detected the limnoria in its borings. Subsequently, the logs of Norwegian pine, laid down to support the temporary railways, were discovered to be very much destroyed. In 1807, when first placed, these timbers were ten inches square, and in 1811, by the ravages of the limnoria, they had been reduced to seven inches, or at the rate of about an inch in the year. And the house-timbers were so much perforated, that, in several instances, they even stood clear of the rock, depending only upon the stanchions and bolts for their support. *

I have already mentioned, that it was from the Bell-Rock that Mr Stevenson first sent specimens of the limnoria to Dr Leach. The same scientific engineer, not long afterwards, having had occasion to examine the timber-bridge of Montrose, found the piles supporting it to be very much destroyed by the borings of the same animal, so that the stability of the whole structure was seriously endangered. Sheathing of the new piles with sheet-copper was adopted as a preservative measure, with partial success. Mr Stevenson saw also the most destructive effects produced by the limnoria on the gates of the sea-locks of the Crinan Canal, which were thereby rendered so inefficient for their purpose, that " the locks lost seven feet of their depth of water in the course of the night."

The next instance of the ravages of the limnoria, of which I know any thing, occurred at Trinity chain-pier, in this neighbourhood. The piles supporting this useful erection were, in 1825,

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* Stevenson's Account of the erection of the Bell Rock Lighthouse, p. 332

(four years after their fixture), found to have been so very much destroyed by the limnoria, as to be useless, and, at great expense, were removed and replaced by new ones. These piles were originally twelve inches square, or forty-eight inches in circumference; and in the course of the few years that elapsed before their destruction became matter of enquiry, the girth of some was reduced to six inches, so that the least force was sufficient to break them through. On this occasion, the means of preserving wood from the attacks of so destructive an animal formed the subject of general attention amongst engineers and men of science; and many plans were proposed and tried. Of these, after two or three years' proof, none was found to answer so well as the covering of the whole surface of the pile, from the place where it met the bottom of the sea, to within a foot or two of mean high-water mark, with broad-headed iron nails, technically called scupper-nails, set close together. Four years afterwards, when it was proposed to extend the pier of Leith on wooden piles, the same plan was adopted, at an expense, for the whole pier, of about L. 1000; the total cost of the pier being L. 30,000. It is satisfactory to know, that, although nearly four years have now elapsed since the first piles of this elegant erection were driven, the limnoria has not effected a lodgement in any of those protected by the iron nails; while several other pieces of wood attached to them, (such as the supports of some hand-rails, and, in particular, the wood-work of a wear running out from the end of the pier to the length of a few hundred feet) are already much destroyed. Some of the last-mentioned timber, which had been originally about two inches in thickness, is almost entirely eaten away. It is worthy of remark, that this is the case, even in the immediate vicinity of iron-bolts and nails; the borings of the animal sometimes approach these within a few lines, yet the piles defended by the nails are not touched. This does not appear to depend altogether upon these last becoming encased with a thick crust of the oxide of iron, impervious to their mandibles; for there are some piles on which this crust has not been formed, close to the wood, tenanted by the limnoria, which do not exhibit the least vestige of its attacks.

It is probable that the animal is prevented from boring in wood covered with iron-nails, by finding the taste of the oxide, which soon covers the interspaces between the heads of the nails, not suited to its palate. The near approach which it makes to iron-bolts passing through wood which it has once attacked, seems to indicate that rust is not absolutely poisonous to it. This was proved also, (as I am informed by Mr James Leslie, civil-engineer), last summer at Trinity chain-pier, when a pile, which had been, sometime before, covered with scupper-nails only on three sides, was taken up, and found to have the whole interior eaten out, only so much of the exterior being left on the three sides as had been penetrated by the nails, and the oxide of iron. I may here allude to the expediency of watching the condition of piles exposed to the ravages of the limnoria, even when covered with iron-nails; because the heads of the nails frequently fall off, after being much oxidated, and it is very possible that the coat of oxide which they have previously communicated to the pile, may also be abraded, so as to leave the wood wholly exposed to the enemy. The nails which fall away ought to be replaced by new ones as soon as possible.

The public prints have recently announced that Mr Stevenson has found a coating of some varnish effectual in protecting wood from the limnoria at the Bell Rock. It is to be feared that the use of such an article could not be depended on in all situations, as it is so liable to be abraded by external violence.

I have now only one other instance of destruction, caused by the limnoria, to allude to particularly. It is met with in certain fir-piles fixed along the base of a stone bulwark, on the sea-shore, in front of Leith Fort. These were erected about thirty years ago, with the view of breaking the waves dashing against the bulwark. They are from six to ten inches square, and are arranged in three lines. The lower ones only have been attacked, and the borings are carried upwards to within three or four feet of mean high-water mark. A few of the piles have been so much eaten away as to have been broken off near their bases by the waves, and almost all are deeply abraded. It would seem to be owing to these piles being so near highwater-mark that they have not been wholly destroyed long ere now. The animals contained in them are rather smaller, and less active than those in wood situated near low water-mark. It is worthy of remark, that some of the Leith Fort piles seem to have been bored from above downwards. This I suppose to have been owing to these having had originally small cavities in their summits, giving lodgement to minute pools of sea-water during the recess of the tide, and thus enabling the borer to continue its work, even within a very short distance of highwater-mark. Such piles are hollowed out in the centre above, as well as eaten away round their bases externally.

I have now adduced a sufficient number of facts to prove that this destructive animal is very abundant on the coasts of Scotland. I have to add, that it occurs also on the English shores, and that Mr Stevenson has witnessed its ravages on the coasts both of France and the Netherlands. Last year, I found it in a small piece of wood, which had apparently been long in the water, thrown ashore in Torbay, Devonshire.

V. *On the purposes which the Limnoria is fitted to serve in the economy of nature.*

Although the destructive agency exerted by this minute animal be forced upon our attention chiefly by its ravages on works of great public utility, and, therefore, cannot at first view be regarded by us with complacency or admiration; yet, upon further reflection, we must be convinced, that the purposes which the limnoria is fitted to serve in the great plan of creation, are such as could be accomplished by no other living creature with which we are acquainted, and that they are calculated to contribute in no small degree to the comfort and well-being of man. Let us consider, for instance, how possible it is that large trees or masses of wood, floated down by rivers, might accumulate on shoals at their mouths to such an extent as materially to diminish the outlet for the waters, which then would rise and overflow their banks, were it not for the destructive boring of the limnoria. What could not be accomplished by the brute force of any marine animal, and might baffle even the ingenuity and power of man himself, is yet quietly accomplished by the gradual but steady operations of a tiny crab. The trees are perforated, and then washed away, and with them the sand and mud which had collected around, and which would speedily have formed an effectual impediment at once to the free efflux of the water, and to the navigation of the river. The limnoria may, perhaps, be likewise instrumental in promoting the disintegration of drift-wood at sea, so as to prepare it for becoming the food of other animals, which have no organs sufficient for reducing it from its original state to one suited to their means of apprehension. In this piece of work, it may be said to be a coadjutor with the teredo and other molluscous borers. But it is worthy of remark, that, through the agency of the limnoria, wood is reduced to much smaller fragments than it is by the Teredo. Nor must we omit to notice the possibility of its being the means, occasionally, of causing the removal of those serious obstacles to the safe navigation of shallow seas which are caused by the masts of sunken vessels.

But, whether we be right or wrong in our suppositions as to the use it may be of in the economy of nature, we cannot be mistaken in regarding the ravages of the limnoria as shewing what formidable enemies to man the Creator can prepare even amongst what appear to us the most insignificant of his works; and how He, who ruleth over all, effects important purposes by means of the smallest, and apparently incompetent, agents.

LEITH, 15*th February* 1834.

Explanation of Plate VI.

FIG. 1. Dorsal aspect of Limnoria, magnified about 10 diameters.
2. Ventral aspect of limnoria. The line between figs. 1. and 2. indicates the real maximum size of the animal.
3. Transverse section of the second segment, shewing the first pair of feet.
4. One of the third pair of feet.
5. One of the fifth pair of feet.
6. Superior pair of antennæ.
7. Inferior do.
8. Lateral view of the head, shewing the position of the eye, and the disposition of its compound corneæ.
9. The two pairs of jaws in connexion.
10. Inner pair of jaws detached.
11. Mandibles detached.

Fig. 12. Diagram to shew the relative situation of the mandibles *a. a.*,—the form and arrangement of the œsophagieal appendices *b. b.*,—and of the intestinal canal *c.*

13. One of the caudal segments, with two pairs of branchial scales.
14. A branchial scale of the external set.
15. A branchial scale of the internal set.
16. Vesicular bodies—functions unknown.
17. Dorsal aspect of fœtus.
18. Lateral aspect of fœtus.
19. Representation of a pile taken up at Trinity Chain-pier five years after it was first placed there. The dotted lines aound the figure indicate the original size of the pile.

INQUIRIES RESPECTING THE WEIGHT OF MAN, AT DIFFERENT AGES. *By M. Quetelet*[*].

The Statistical Researches of M. Quetelet are well known to the public. They possess the rare merit of at once being very exact and well arranged, even in the most minute details, and in investigating certain very important scientific and philosophic questions. Accordingly, in estimating, with more precision than had been done, the weight and the average stature of the male and female, at different ages (or different periods of life) and among different nations, M. Quetelet, as well as M. Villermé and other *savans*, have not been desirous to satisfy a vain and trifling curiosity; they have deduced principles relating to the health of man in different circumstances, to Hygiene, to the laws of conscription, and even to the fine arts. Under the last relation, which appears a little foreign, this is the manner in which M. Quetelet expresses himself in the preamble of the work of which we speak.

"In order to produce a work which may be truly capable of touching us, and of affecting our passions, we must know man, and above all, the man whom we would represent. To take a single example, the artist, who has studied only the type of the

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[*] A Pamphlet in 4to, of 43 pages. Brussels 1833.—Translated by the Rev. William Ettershank.

Greek physiognomies, however admirable in other respects this type may appear to us, if he reproduces it in modern subjects, it will be cold and ineffectual upon the spectator, who will perhaps admire the art and composition, but will not be deeply affected. The Greek figures, however varied they may be, on account of age, passion, and sex—have, notwithstanding, all an air of a family, which carries us back towards antiquity, in spite of ourselves, and withdraws our attention from the subject which we would represent.

"If one endeavours to make them, the anachronism will become more sensible. Artists at the commencement have comprehended this necessity of painting what they had under their eyes, and it is in this way that they have produced effects so magical. The noble and severe figure of Christ has nothing in common with that of Apollo nor of Jupiter of the ancient mythology. A virgin Mary of Raphael has an enchanting grace which yields to nothing in the most beautiful ancient forms, and they exert on the imagination a greater influence, because they are more conformable to nature by which we are surrounded, and because they act more immediately upon us. We ourselves in climates more remote, perceive the need, in retracing our national actions, not to present Greek or Italian figures in the midst of a battle, in which we find men, almost all of the same age, all alike covered with warlike apparel, our eye seeks to recognise, from the features and expression of the physiognomies, the Frenchman or the Englishman, the German or the Russian. Even in the French army, the soldier of the Old Guard had a physiognomy which had become classic, and which was identified in some measure with the remembrances of the empire.

"It is certainly to the little attention that has been given to the study of the shades, by which the physical and moral qualities of man pass among different nations and in different ages, which gives rise to that monotony and coldness of the greater part of the works of the imagination. We have, indeed, perceived the necessity of studying nature, and of being true; but, I think we have not remarked sufficiently that nature is not invariable. The ancients have represented, with infinite skill, physical and moral man, such as he existed then; and the greater number of the moderns, struck with the perfection of their works, have believed that they can do nothing better than servilely imitate them; and they have not considered that the type had changed, and that, in imitating for the perfection of the art, they had another nature to study. Hence, this universal cry, 'Who will deliver us from the Greeks and the Romans?' hence this violent schism between the classics and the *romantique;* hence in fine, the need of a literature which would truly be the *expression of society.* This great revolution is accomplished, and it furnishes a proof the most unexceptionable of the variability of the human type, of *l'homme moyen*, among different nations and in different ages.

"Thus the determination of average man is not useless, even for the fine arts and letters, and he who would arrive at this determination, will have no difficulty to make artists and literati listen to him. He would teach them to know, in a more precise manner, things which they already know vaguely; he would teach them other things of which they are ignorant, or at least he would rectify their judgment concerning a multitude of prejudices. They would receive these, in the same manner as a painter learns perspective, which, under its geometrical form, is far from being picturesque also. Moreover, they have received the researches of Gall and Lavater, with more eagerness perhaps than the *savans* themselves; it is even to their care that we owe, in a great measure, the knowledge of the proportions of the different parts of the human body as respects age and sex."

In the little work in which we find these general considerations, with others which we are compelled to omit, the author investigates what concerns the development of the weight of man in the same manner as he has determined his growth, his inclination to crime, the succession of generations, &c. Afterwards he will publish new inquiries concerning the strength, swiftness, and other qualities of the human species; inquiries which, in order to be exact, must be made by many associated observers, and upon a great number of individuals. Physicians and engineers have been sometimes led to estimate the weight of men arrived at maturity, and considered, for example, as burdens placed upon a building, or as weights acting on a machine. La Hire has made very remarkable researches of this nature. On the other hand, the legal practitioner must often be occupied with this subject, for one of the most frequent problems, is to determine after death, the probable age of an individual, from the assemblage of physical qualities. On this grave question, we are generally reduced to the estimation of practitioners more or less vague; but if, in a case of infanticide, for example, we stated in the *proces-verbal* the weight and stature of the infant, as well as those physical characters susceptible of measure, and as we might have by the side of that, well constructed tables, which would give us, for the different ages, the values of these physical qualities and their mean variations, we would have terms of comparison which would be better than the appreciation of practitioners, or which would serve at least to control their assertions. We see from these examples, that the inquiries concerning the weight of man, have more than one application.

The observations of M. Quetelet were made at Brussels in the Maternal Hospice of St. Peter; he compares them with those made at Moscow and Paris, in similar hospices; and he finds little difference between the means obtained. Unfortunately the Russian and French practitioners have not distinguished, with as much care as M. Quetelet, the sex, the stature, and the weight of children observed at their birth. This renders the results less capable of comparison. M. Quetelet found for sixty-three male children, and fifty-six female, newly born, the following quantities,

	Weights.	Stature.
Male Children	7.057536 lb. Avoird.	1.62732 feet
Female ——	6.4179468	1.58467

The extremes are:

	Boys.	Girls.
Minimum	5.1608232 lb. Avoird.	2.4701376 lb. Avoird.
Maximum	9.92466	9.36329

The mean weight without distinction of sex, is 6.7377414 lb. Avoird. It has been found at Paris on 20,000 observations 6.74656332 lb. Avoird.*

M. Quetelet has made similar inquiries concerning children from four to twelve years of age, in the schools of Brussels, and in the orphan hospital,—concerning young people in the colleges and in the medical school,—finally, concerning old men in the magnificent hospice which has been constructed in the same city for a period of four years. The results have been completed by observations made upon isolated individuals, taken by chance from the mean of all these data. The author has been able to construct tables, which shows the mean stature, the mean weight, as well as the minimum and maximum, at each age, and for both sexes. These tables show, that there exists at each age, and for each sex, a constant relation between the mean weight and the mean stature, from which the author has constructed another table, more exact than those which result directly from observations on the weights. It follows, from the mean stature formerly observed by the author, for the whole population, or at least from a number of individuals much more considerable than in these last inquiries, and gives the mean weight corresponding to each stature, according to the observation which make the subject of this memoir.* The following is a table, which we may consider as exact for the whole population of Brussels, and which, for want of a table of this sort, calculated for other countries, may serve, at least, as an approximation for the Caucasian race, and in a temperate climate.

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* Dictionary of Medical Science, article Fœtus.

We see, from this table, 1*st*, That, at an equality of age, the male is generally heavier than the female; towards the age of twelve years only, an individual of either sex has the same weight. 2*dly*, That the male attains the maximum weight about the age of forty years, and that he begins to lose, in a very sensible manner, towards his sixtieth year; that, at the age of 80 years, he has lost about 13.23288 lb. Avoird., the stature being also diminished 2.75604 inches. 3*dly*, That the female attains the maximum weight later than the male, towards the fiftieth year. 4*thly*, That when the male and the female have assumed their complete development, they weigh almost exactly twenty times as much as at the moment of their birth, while their stature is only about 3¼ times beyond what it was at the same period.

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* The author cannot consider the results obtained in hospitals and public schools as very exact, as to the mean stature of the population, because inquiries made by him concerning a great number of individuals have proved to him that the mean stature is a little more among individuals in easy circumstances than in the indigent population, who have recourse to hospices, hospitals, and gratuitous schools.

A SCALE OF THE DEVELOPMENT OF STATURE AND WEIGHT.

AGES.	MALES.		FEMALES.	
	Stature.	Weight.	Stature.	Weight.
Years.	Imp. Feet.	℔ Avoirdupois.	Imp. Feet.	℔ Avoirdupois.
0	1.64045	7.05736	1.60764	6.4179468
1	2.29007	20.841786	2.26382	19.3861692
2	2.59519	25.0101432	2.56238	23.5324716
3	2.83469	27.5023356	2.79532	26.0026002
4	3.04468	31.3839804	3.00102	28.67124
5	3.24153	34.7804194	3.19559	31.6706928
6	3.43511	38.7982752	3.38261	35.28768
7	3.62539	42.984168	3.56305	38.6841192
8	3.81240	45.7857648	3.74351	42.0805584
9	3.99942	49.954122	3.92067	47.1040528
10	4.18314	54.0783696	4.09457	51.8728896
11	4.3636	59.768508	4.26189	56.570562
12	4.54404	65.7674136	4.43905	65.7674136
13	4.72122	75.8244024	4.60310	72.6485112
14	4.89838	85.4844048	4.76714	80.941116
15	5.07227	88.69745824	4.91807	89.035276
16	5.22975	109.5461916	5.03618	96.0927636
17	5.36099	116.559618	5.10179	104.3412588
18	5.43973	127.587018	5.13132	112.5456444
20	5.49222	132.4611288	5.15757	115.3024946
25	5.51191	138.7909564	5.17398	117.5079744
30	5.52503	140.378802	5.18054	119.8237284
40	5.52503	140.4229116	5.18054	121.8086604
50	5.49222	139.9597608	5.03946	123.8597568
60	5.37740	136.074312	4.97384	119.757564
70	5.32490	131.2701696	4.96728	113.6042748
80	5.29219	127.5429084	4.94103	108.884576
90	5.29219	127.5429084	4.93775	108.8183832

Children lose weight during the three first days after their birth; at the age of a week they begin sensibly to increase; after one year, they have tripled their weight;—then they require six years to double the weight of one year, and thirteen to quadruple it.

To calculate the burden of an edifice, or a bridge, covered with a crowd, it is well to know, that the mean weight of an individual, whatever is the age or sex, is about 98.584956 lb., that is, 103.65756 lb. Avoird. for the males, and 93.7328 lb. Avoird. for the females.

During the development of individuals of both sexes, we may regard the squares of the numbers representing the weights, at the different ages, as proportional to the fifth powers of the sta-

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* Here the meter = 3.2809 imperial feet, which was used in the reduction, and 2.20548 was used in weight, which is slightly greater than 2.20486, is preferred, though the difference is not worth minding.—*Galbraith.*

tures. After the complete development of individuals of both sexes, the weights are almost the squares of the statures.

The weights have varied, in the extremes, among individuals regularly conformed, from 1 to 2, whilst the stature has varied only from 1 to 1⅓.

The inferiot parts of the body are developed more than the superior. In a child, the head is equal to a fifth part; and in a full-grown man, to an eighth of the whole height of the individual. It appears, from a note at the end, that these proportions vary a little among different nations; but M. Quetelet, who, in his preliminary observations, explains very well the importance of these inquiries to the fine arts, does not appear to have written this work purposely fot them. The activity which he has exhibited in his researches, make us believe and hope that he will resume them at another time.

This little interesting work is terminated by data concerning the weight of human bones, which belonged to individuals of different ages, a very important subject in certain cases of legal medicine.

CUVIER AS A NATURALIST.

By C. L. LAURILLARD, Conservator of the Cabinet of Anatomy in the Museum of Natural History of Paris.

> The lives of the learned proclaim to us on every page, that great truths have never been discovered and established, but by prolonged and solitary study, constantly directed to a special object, and continually guided by a severe and cautious logic.—CUVIER, El. Hist. *

IF men of superior genius had not appeared from time to time in the world, and imparted to society the energy of their own minds, the latter would still have been in a state not far removed from infancy. After having provided for their most pressing wants, men would relapse into their natural indolence, and make no effort to attain perfection; and the state of ignorance in which we find many nations at the present time, would still have been the condition of the whole human race. But such was not our destiny: it was the will of the Supreme Power that our knowledge should be continually extending; and, to attain this object, He has provided that creative genius should sometimes rise in the midst of us, to circulate new ideas and unveil new facts,—to lead us into new paths, and stimulate us to additional labours, by the allurements of curiosity, or the prospect of new enjoyments.

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* We recommend to our readers Mrs Lee's interesting Biography of Cuvier, lately published in one volume 8vo.—EDIT.

The history of the sciences teaches us, that many of the instructors of the human race, and men whom destiny has raised to give an impulse to the age in which they live, appeared together at different epochs, as if mutually to stimulate each other by a noble emulation, without which, perhaps, the fire that animated them would not diffuse so bright a lustre.

The close of the last century was one of the most brilliant of these epochs; in almost every department of knowledge it has given birth to great men, who have opened new paths to our minds, furnished new materials for thought, and given new scope to our imaginations; and no one will doubt, that the individual whose eloge we now undertake, was one of those who shed on it the greatest lustre, by the sagacity of his views, the extreme clearness with which he explained his ideas, the immense extent of his knowledge, and the importance of the truths which he revealed.

At the time when his first writings appeared, no naturalist, perhaps, thought that Zoology was a subject that afforded the means of rendering a name famous. It seemed as if Linnæus, by his precise and simple systems, and Buffon, by his animated pictures, bold views, and that union of science with eloquence, unknown before his time, had exhausted the subject; but to a man of genius Nature is an inexhaustible source of study and reflection. By applying the principles of the natural method to the classification of animals, M. Cuvier entered upon a zoological career not less brilliant and extensive than that of these two great men.

Up to his time, although the subject had occupied Camper, Blumenbach, Hunter, Daubenton, and Vicq-d'Azyr, comparative anatomy had scarcely been more than an object of curiosity, and of more or less ingenious dissertation; M. Cuvier erected it into a science, which became, in his hands, the ground-work of Natural History, and the most fertile source of physiological truths.

The labours of Saussure, Deluc, Pallas, Werner, &c. appeared to have brought Geology to that degree of perfection it seemed capable of attaining; but by the discovery of a kind of monuments which living nature has left in the bowels of the earth, M. Cuvier originated in this science a new order of ideas, which have changed the character of its philosophy.

Such is an abridgment of what this modern Aristotle has done for zoology, comparative anatomy, and the history of the earth.

It may seem that the labours entailed by giving a new impulsion to three branches of natural science, would have sufficed to give occupation and celebrity to the longest life: but this was not enough for the vast mind of Cuvier. Historian of science and of the academy, an eloquent professor, and author of many immortal works, he wished, moreover, to render more immediate services to his cotemporaries, by connecting himself with the body that superintended the instruction of youth; and the talents for business which he displayed in this capacity, caused him ultimately to be called to take a part in the deliberations of the Council of State, and, some years afterwards, to preside over one of the sections. It was thus that he was drawn into a path which the events of our days have beset with so many difficulties, and in which, experienced as he was in searching for truth, animated with that religious regard for justice which the rectitude of his character inspired, enlightened by a profound knowledge of administration, and guided by an integrity worthy of primitive times, he has acquired, as a politician, perhaps as great a celebrity as he did in the character of a man of science.

It will readily be understood, that his numerous works were not published in any regular succession according to the nature of the subjects; and as we are not writing his biography, but his eloge, that is an appreciation of his merits, that we may not tire the reader by frequent recurrence to the same subject, we shall not follow them in chronological order, but in the order of the subjects as they have been briefly indicated, and attempt to characterize what he has done in each; but as long details of his life would prevent this, we shall mention only such circumstances as appear indispensable.

GEORGE CHRETIEN FREDERIC DAGOBERT CUVIER was born on the 23d August 1769, at Montbéliard, the chief town of a principality belonging at that time to the Dukes of Wurtemberg. His parents were by no means in affluent circumstances, and, like many of the eminent men whose history he has delineated, he had to struggle in his youth with misfortune.

His father, after forty years of distinguished service in a Swiss regiment in the pay of France, found himself possessed of only a moderate pension for the support of his family; and the young Cuvier, that he might not be a burden to his parents, was obliged, immediately after completing his studies, when about the age of nineteen, to engage in the humble occupation of tutor in an ancient family of Normandy. This step, which such of his friends as could appreciate his rising genius regarded with sorrow, proved, notwithstanding, the origin of his fortune. In this way destiny often leads us to success by a path which, to our limited views, seems to remove us to the greatest distance from it.

Endowed with a memory of extraordinary power, and a singular aptitude for every kind of intellectual labour, he had shewn from a very early period a decided taste for natural history and drawing, the latter of which he always regarded as the most certain means of facilitating the study of this science. A copy of Buffon having fallen into his hands, when he was about ten years of age, he read it with avidity, and never rested till he copied all the figures, and coloured them according to the descriptions.

His taste for these studies was confirmed at the academy of Stuttgard, where he had been placed by Duke Charles of Wurtemberg, on the report of his brilliant talents. While he studied every branch taught in this celebrated institution, philosophy, mathematics, law, &c.; he attended more particularly to the course of natural history, and occupied himself during his walks in forming an herbarium and a collection of insects. His intercourse with his fellow-pupils, particularly with M. Kielmeyer, the father of natural philosophy, contributed largely to his rapid progress in every department of natural history.*

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* It is a circumstance worthy of remark, that M. Cuvier (in some respects the disciple of Kielmeyer, for being younger, he greedily treasured up his conversations, and learnt from him the art of dissection,) may be consi-

When he arrived in Normandy in 1788, the facilities which his vicinity to the sea afforded him, gave rise to the desire of studying its productions. He first examined the animals of the class Vermes of Linnæus, but finding difficulty in determining the species and even the genera, as then established, he inquired whether their internal structure might not supply more precise characters; and the necessity of classifying the facts which he ascertained, soon made him sensible that it was impossible to assign any common character to these animals, and that a desideratum in zoology therefore remained to be supplied.

Linnæus and Buffon had made science popular; the one by rendering it of easy attainment, the other by giving it a philosophical character, and investing it with all the fascinations of eloquence; but this very popularity having been the means of enriching it to a great extent, a method of classification became indispensable; and as the systems founded on a single organ could not lead to an arrangement of animals according to their affinities, M. Cuvier conceived it necessary to apply to zoology principles analogous to those of the natural method, recently employed with so much success in botany, and which consist in "distributing the facts of which a science is composed in proportions, so graduated and arranged in their generalities, that when taken together they will express the real relations of beings *."

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dered as representing, among naturalists, the Peripatetic school, in its method of observing facts, comparing them, and thence deducing general principles; whilst Kielmeyer is the founder of a metaphysical school, closely resembling the academic, inasmuch as it admits the independent existence of general ideas. In him, then, we find another Aristotle, disciple of another Plato, each pursuing a tract as distinct as that of the Greek philosophers.

* Cuvier, *Rapport historique sur les Progrès des Sciences Naturelles*, Paris, 1810, p. 304. Such was not the method in the Linnean system, which was defective especially as it regarded the lower animals. Vertebral animals divide themselves so naturally into classes, that an instinctive *feeling* has led all zoologists since the time of Aristotle, to recognise those which prevail even at present; but resting only on this feeling, which reasons not, and therefore never inquires after the foundation of its apprehensions, this natural division of the classes had never been carried further; the subdivisions into orders and families required observations and reflections, which had not generally been made. A better distribution of the mammiferæ, though still incomplete, had been proposed by Storr; but the other vertebral animals required a reformer to arrange them, as much as the invertebral. Their forms being much more varied, analogy, neither by reflection,

As the animals of the class Vermes, which Linnæus had arranged in such a singular manner, were most accessible to him in his present situation, it was on these that he resolved to make the first trial; but before publishing any thing on the subject, he wished to consult the most renowned scientific men of the time, and opened a correspondence with some of them through a member of the old Academy whom he met at Valmont, at whose desire he had been induced to deliver a course of lectures on botany to some amateurs in Natural History. MM. Millin, Lacepede, and Geoffroy Saint-Hilaire invited him to Paris. He went thither in 1795; and the lectures which he delivered to the Philomathic and Natural History Societies, various memoirs on the anatomy of the mollusca, insects, and zoophytes, together with a sketch of the advantages and best method of forming a system in natural history, placed him in the first rank of naturalists, and caused him to be nominated a member of the Society of Arts, professor to the central school of the Pantheon, and soon afterwards member of the Institute, and assistant to the professor of comparative anatomy in the museum of natural history.

Placed in the centre of this vast establishment, and having the objects which it contained at his disposal, his genius was displayed in a manner proportionate to his means of investigation.

Still pursuing the anatomy of the mollusca, he published the result of his labours in a series of monographs, which will ever form a model of clear and precise description, learning and literary criticism, while they shew his singular skill in the art of drawing and making anatomical preparations.* He extended his researches to other invertebrate animals, and, in 1796, made known to the world his beautiful discovery of the circulation and red colour of the blood in leeches and other annelides. In 1797, he read his celebrated memoir on the nutrition of insects,

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nor by instinct, could conduct to their arrangement; and that which Linnæus adopted, corresponded less with nature than that which was before proposed by Aristotle.

* M. Cuvier, from memory, sketched all natural objects with the greatest rapidity, depicting their general characters, and the relative proportions of their parts, with an accuracy that excited the admiration of all his auditors, as upon a black board he made a figure to appear rather as if it were by magic, than by tracing it with chalk.

in which he established, in the most logical manner, the mode of respiration by tracheæ, and of absorption by imbibition—peculiarities occasioned by their want of circulation. This memoir subsequently led to the separation of insects from other articulated animals.

In the course of these labours he was gradually bringing together a numerous collection of materials, which enabled him to place comparative anatomy on a secure foundation—to effect the discovery of an ancient zoology—and introduce a complete reformation into the whole system of animated nature. This reformation was begun in his elementary table, or synopsis of lectures in the central school of the Pantheon*—carried still farther in the tables printed at the end of the first volume of his lectures on comparative anatomy,—brought nearer to perfection in 1812, in a memoir inserted in the Annals of the Museum of Natural History†, and completed, as far as he had it in his power, in two editions of his Animal Kingdom ‡.

According to the principles of these works, now generally diffused and established through the influence of his writings and oral discourses, the natural history of an animal is the knowledge of all its relations and properties, and its organization must determine its place in a methodical arrangement: anatomy and physiology, therefore, must form the basis of zoology, and the most general and constant facts in organization must determine the great divisions, while those of a less general and more variable kind will serve for secondary divisions. He thus established a subordination of character and sections, which alone can be the principle of a natural method, that is, of arranging animals in such a manner that the place which each of them occupies affords a general idea of its organization, and the attributes which connect it with others.§ This method he regarded as science itself reduced to its most simple expression.

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* One vol. in 8vo. Paris. † 19th Vol.

‡ Four vols. in 8vo., Paris, 1817. Second Edition, 5 vols. in 8vo., Paris, 1829. Entomology, like all the other branches of natural history, having within a few years received large augmentations, it was not possible that a single individual could thoroughly investigate the whole series of animals. For the publication of this work, M. Cuvier united with M. Latreille, who undertook the Crustacea and Insects.

§ No system that is founded upon the observation of a single organ, how-

Examining in this manner the modifications which the animal kingdom presents in the organs of circulation, respiration, and sensation, instead of the six classes of Linnæus, viz. quadrupeds, birds, reptiles, fishes, insects, and worms, M. Cuvier established four great types—vertebrate, molluscous, articulated, and radiated animals, which he named *embranchemens*, and which he divided into classes nearly equal in value to those which had been established among vertebrate animals.

Many of the inferior classes were thus raised to a place of considerable importance; but, from the time of Linnæus, it had been understood that neither size nor utility ought to have any influence in scientific distributions; and the soundness of the reasons on which M. Cuvier acted has caused his views to be generally adopted, scarcely a voice being heard in favour of the old modes of classification. So limited, moreover, is our knowledge of the designs of the Author of nature, that animals which appear of little importance in relation to ourselves, are perhaps as necessary to the general plan of the Creator as those which we place at the top of the scale of beings.

To examine the division of the orders, families, genera, and all the subordinate details, would lead us to exceed the limits to which we are confined. Let it suffice to say, that the principles on which these divisions are founded, will necessarily remain unaffected by the changes which new observations will render necessary,—that the basis of every zoological classification are henceforth fixed,—and that their solidity will prove to future naturalists better than any eulogium, the lofty genius of the author of the Animal Kingdom.

This work will doubtless undergo the fate of all other scientific productions. The spirit in which it is conceived will alone continue unchanged. There is no naturalist of the present day

ever important it may be, can lead; like this other, to the formation of families based upon their real nature, because there is none of them that does not interfere with the natural relations. Besides, as it is the natural habit of our minds that, in the course of time, the system comes to be considered as an essential part of the science, this essential part is so reduced, when it derives its characters from the modifications of an individual organ, that the works which it originates are usually of little value; whilst the natural method founded on the analysis of all the organs, neglects none of them, and produces valuable works only in which every theory may find the elements on which it rests.

who does not possess more knowledge than Aristotle,—no pupil of the Polytechnic School less knowing than Newton,—and perhaps the vast science of Cuvier will one day be surpassed by that of every student; but Aristotle, Newton, and Cuvier, will not, on that account, be less honoured in every age.

These works on classification gradually became more perfect, as new facts in anatomy came to his knowledge, and suggested to him new ideas; and his incessant activity speedily enabled him to lay them before the public. Scarcely four years had elapsed since his nomination to the museum, when he commenced the publication of his immortal Lectures on Comparative Anatomy,—a measure become absolutely necessary for the direction of the numerous pupils which the rising reputation of the professor attracted to his course.

In the first general work written on this subject,* which forms an indispensable guide to all who study this extensive science, M. Cuvier, instead of considering the anatomy of each animal separately, examines in succession each organ in the whole series of animals, taking accurate notice of the various modifications and changes which they undergo, in order to deduce from them the general theory of their functions.

A circumstance which ought not to be separated from the mention of this work, and what constitutes one of his fairest titles to honour, is the formation of a cabinet of anatomy. Of all that he taught in his lectures, he wished to supply demonstrative proofs. For this purpose he collected the preparations of Daubenton, and those belonging to the old academy of sciences which he found much mutilated and in disorder. These he rapidly augmented by his own incessant labour, and by giving a proper direction to the exertions of his assistants, till he succeeded in establishing this collection, which still remains the richest in Europe, in spite of the efforts which some other nations have made to equal it. In it not only were his own pupils formed, but rivals from other countries came to derive information from the matchless preparations, many of which were made by his own hands.

It would be equally impossible to indicate here the portions of the work on anatomy which belong exclusively to M. Cuvier.

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* Published in 5 vols. 8vo, 1800–5.

He has no doubt availed himself of every author who has treated of animal anatomy; he has profited by the works of Swammerdam, Collins, Monro, Hunter, Camper, Blumenbach, Daubenton, Vicq-d'Azyr, and many others; but a multitude of new and important facts are due to himself, and what is peculiarly his own, is the elevated manner of considering the subject, the vigorous precision with which he traces one organ through the whole series of animals, the patience he displays in marking their differences and the effects which they ought to produce, his logical accuracy in deducing only such consequences as flow directly from the facts, without giving way to the seductions of a system; and finally, that perspicacity and condensation, of which he has given a striking proof by comprehending in a single lecture on animal economy, the substance of many volumes written on the subject.

The principal physiological ideas which this, as well as the other works of M. Cuvier, contains, are that life is a *tourbillon* of a certain matter under a determinate form; that the principal agent of it is an imponderable fluid, the nervous fluid; that the sensation and reproduction of living beings are subjects incomprehensible to us; and that instinct is an internal sensation, a sort of somnambulism which determines certain animals to execute, in a state of ignorance, actions often very complicated, without having been taught by others or by experience.*

If the anatomy of the molluscæ led M. Cuvier to reform zoological systems, the anatomy of the vertebrate animals occasioned the discovery of an order of facts, even more fruitful in brilliant results for natural philosophy and the theory of the earth.

Considering that an organized being forms a complete system, destined by nature for a certain purpose, and of which all the parts are intimately connected with each other, he conceived that there existed so necessary a corelation of form between these parts, that none of them could be modified without influencing all the others, and that each modification was in itself sufficient to make known all the rest; he thence concluded, that every bone of the skeleton of an animal bears the character of the class, order, genus, and even of the species. Applying this doctrine to determine the bones scattered among the different strata of the earth, he discovered what had escaped the observation of Camper and Daubenton, who likewise applied comparative anatomy to the determination of fossil bones, that these remains of animals belong to extinct races, different from those which now exist. *

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* M. Cuvier, considering that all organized beings are produced by generation, and not seeing in nature any power capable of producing organization, believed in the pre-existence of germs; not in the pre-existence of a being fully formed, because it is very evident that it is only by successive developments that an animal acquires its form: but if it may be so expressed, in the pre-existence of *the radicle of a being,* a radicle which exists before the commencement of the series of evolutions, and which certainly mounts up, according to the interesting observations of Bonnet, to at least many generations.

This discovery, in the sequel of his researches, † led to the knowledge of another fact not less unexpected, that the differences between fossil and living animals, increased in proportion to the age of the deposit which contains them, in such a manner that the exposition of these differences becomes a kind of chronological table of the deposits.

Let us take a rapid survey of the most general consequences which flow from these new facts.

As the primitive rocks, on which all the others repose, contain no remains of living beings, we thence infer that the latter did not always exist on our planet. Whether the too high temperature prevented it, or the materials necessary to organic existence were not prepared, there was a time when physical forces acted alone on the rocks and on the seas, where the wonders of organization were developed at a later period.

All organized beings have not been created together; vegetables seem to have preceded animals; the molluscæ and fishes

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* The number of the species of the fossil Vertebral Animals recognised by M. Cuvier, amount to 168, and form about 50 genera, of which 15 at least are new. Many new genera have since been discovered; and when we reflect how few localities have been examined with care, we are led to believe that the lost species are perhaps more numerous than the living ones. The extensive labours now making on fishes, on shells, on the madrepores, and on fossil plants, yield truths of the same kind with those of Cuvier on the mammifera and reptiles.

† Published in separate Memoirs in the Annals of the Museum, from their commencement to the year 1811, and collected in 4 vols. 4to. in 1812, and in 5 vols. in 1821.

must have appeared before reptiles, and the latter before mammiferous animals.

The species which anciently inhabited the earth have been destroyed and replaced by others, and the present race is perhaps the fourth.

Geology thus has a guide through the obscure labyrinths which it is obliged to trace, and a new means of determining the nature of deposits, which often cannot be attained by chemical analysis, and by the order of superposition.

Besides the general facts which naturally arose from these discoveries, and which M. Cuvier has discussed in the preliminary discourse to his work, with that power and logical acumen which were peculiar to him, science speedily obtained positive results of the greatest importance; for, scarcely had this geological guide been detected, than it led its discoverer to perceive that the stratified deposits of the crust of the globe were divisible into two classes, one formed in fresh water, and the other in the water of the sea. This distinction, which could only be made by means of zoology, has demonstrated a fact not less curious, that many parts of the surface of our earth have been covered alternately by the sea and by fresh waters.

All that we have said of the cabinet of anatomy, we might here repeat regarding the immense collection of fossil bones which he collected, and generously deposited in the museum. He did even more than collect and describe them; with the intention of affording even to the most incredulous an opportunity to feel and believe, he caused models of the principal pieces to be sent to different cabinets throughout Europe. This plan being promptly imitated, has already produced a kind of commercial exchange, extremely beneficial to science, and which, there can be little doubt, will daily continue to extend.

An important problem now presented itself for solution. Since remains of our present animals are nowhere to be found, has man been recently placed on the earth, or rather was he contemporaneous with the destroyed mammifera, and did he escape destruction by his numbers and superior intelligence? According to M. Cuvier, geology proclaims that man is new, at least on our present continents. He declares that in no regular deposits are human bones to be met with—all those formerly regarded as such, have been ascertained to belong to animals; those now found come from the fissures of rocks, and the soil of caverns, dug by the hand of man. But these negative proofs did not satisfy his mind; persuaded that there ought to be others of a positive kind, he sought to supply them from history. By submitting to a rigorous criticism the pretended documents which refer human societies to a very remote period, he demonstrates, or at least seems to us to demonstrate, that positive historical traditions do not go further back than five or six thousand years, a date which various geological phenomena equally concur in assigning to the last revolution; and, that if man existed at that revolution, it was not upon the present soil, but upon a soil carried off by a catastrophy which has spared only a few individuals of the different races now scattered over the surface of the globe.

Some recent works, which have acquired a just celebrity, and which the premature death of their author has terminated in such a deplorable manner, seem to prove a contrary doctrine. But notwithstanding the authority of M. Champollion's name, we may be permitted to question the accuracy of his opinions, as long as they remain without irrefragable proof, since M. Cuvier, with his vast erudition, and almost instinctive perception of truth, was of opinion that the reasons for assigning a more remote origin to society, did not rest on a secure foundation.

The book containing these profound researches, immediately acquired, like his Comparative Anatomy, Animal Kingdom, and Anatomy of the Molluscæ, a classical reputation, and will no doubt support that character, as long as man seeks for enjoyment in the study of nature. It will always form a model of criticism and rigorous analysis, a perfect example of the talent which consists in saying in a few words all that it is of importance to know; together with a power of condensation, which can be possessed only by one having the most extensive knowledge of things, and which M. Cuvier always displayed in the highest degree. In these works there is neither redundancy nor digression, and there is likewise no dryness or omission. We have seen young naturalists reading with pleasure the anatomy of the molluscæ, or the osteological descriptions in his researches on fossil animals; and we have witnessed students consulting what he says on human anatomy, in order to procure more lucid explanations than those contained in the text-books of the schools. In short, throughout his whole writings are to be found these tests of true genius, profundity, perspicacity, and precision. His first lecture on Comparative Anatomy contains all that is known on organization and physiology; his Introduction to the Animal Kingdom presents a most distinct analysis of his zoological distributions, together with all that can be said on systems; finally, the preliminary discourse to the fossil bones gives an admirable exposition of the principles of the harmony of forms, and all the theories on the formation and revolutions of the globe. He gives, at the same time, a view of the historical documents which are thought to authorise the opinion of the high antiquity of different people.

Hitherto we have been so occupied by the examination of M. Cuvier's works, that we have neglected to speak of the position in society to which their celebrity raised him: it affords an example to youth, inciting them to labour, and not to allow themselves to be overcome by discouragement. He was raised to the professorship in the Museum of Natural History, and became successively professor to the College of France, member of the Legion of Honour, member of the Institute, one of its first annual secretaries, and perpetual secretary; inspector of studies, counsellor of the Imperial University, master of requests, counsellor of state, grand officer of the Legion of Honour, one of the forty of the French Academy, honorary member of the Academy of Inscriptions, member of all the learned societies of the world, and finally, a peer of France.

A lengthened list of his occupations might produce the belief that ambition, as if to furnish a new proof that genius is not exempted from human weaknesses, held this master of science in subjection to her yoke. To such as are disposed to entertain this opinion, we may mention, that he many times refused the appointment of Superintendent of the Jardin du Roi, rendered so celebrated by Buffon, and that, a short time before the death of M. de Richelieu, he refused the portefeuille of the minister of the interior. So far was he from soliciting places and honours, that some of them were conferred on him in his absence. He was at Marseilles when he was elected perpetual secretary of the Institute; at Rome, when nominated master of requests; at London, when the French Academy admitted him among their number; in Holland, when he received a donation from the Emperor; and it was by the spontaneous wish of Louis XVIII. that he was created a Baron. If he accepted this unsought for title of honour, it was no doubt from the feeling that it would have evinced more vanity to refuse it; for, it must be admitted, that more pride than humility or philosophy is shown by those who reject distinctions, which the wise accept without considering themselves more elevated, and which the proud refuse to increase their consequence.

This modest instructor was not only invested with all scientific and literary honours, but was often intrusted, in the exercise of his important functions, with missions of the most delicate nature. As inspector of studies, he was commissioned to organize the Lyceums of Bordeaux, Marseilles, and Nice; as counsellor of the University, he was at the head of the commissions sent into Italy, Holland, and Lower Germany, to connect with the Imperial University the establishments of public instruction beyond the Alps and to the north of the Rhine. At a later period he directed the commission of public instruction; and during many years presided over the faculties and the primary protestant schools. Finally, as counsellor of state, he presided in the committee of the interior, and directed the forms of worship which were not catholic.

These journeys, besides producing several writings, which shall afterwards be noticed, contributed to complete his work on fossil bones, for he every where hastened to visit the collections, and to cause drawings to be made of every thing that attracted his attention. It was in this way that he detected a great number of the bones of hippopotami in the cabinets of Tuscany, and that at Leyden he could verify his conjectures respecting the great aquatic salamander of Œningen. These journeys likewise proved beneficial to M. Cuvier's health, by diverting his mind from the labours which have been mentioned, as well as others of which it remains for us to give some account.

As perpetual secretary, he had to pronounce an eloge on the deceased members, and to give an annual account of the labours of the Academy; and, in 1808, he had, in addition, to present to the Emperor a picture of the progress of science from 1789. This was to meet a vast idea of Napoleon's, who wished to appreciate all that had tended to produce the great impulse given to men's minds at this period; and it may be affirmed that the manner in which the task was executed equalled the elevated views which gave rise to it. The connexion of the facts, exactness in analyzing the works of others, and extreme perspicuity displayed in this sketch, as well as the discrimination with which he assigns to each his due, concur to place his work among the first on the history of science; and we may believe that he who suggested it congratulated himself on its execution.

The qualities just mentioned were conspicuous in his analysis of the works of the Academy of Sciences, and in his numerous reports or Memoirs presented to this Academy; but especially in his Eloges, where he succeeded, even better than Fontenelle, in adapting science to the general taste of mankind, and in delighting his reader with simple pictures of the life of men, almost all of whom were obliged, like himself, to overcome in their youth the difficulties which fortune had thrown in their way, and by reflections marked by a wholesome philosophy, which he was in the habit of drawing from the subject.

All these merits, viz. a discriminating analysis, precision, and perspicuity, wholesome and elevated conceptions, which had their source in a profound knowledge of things, he shewed very conspicuously in a work which unhappily exists only in the recollection of his hearers; we allude to the history of the natural sciences which formed for some years the subject of his lectures in the College of France, and of which we can here indicate only the fundamental idea, that, as society could not be developed but by means of the discovery of the properties of natural bodies, each of these discoveries corresponds to a new degree of civilization, and that the history of this civilization, and consequently of every thing relating to the human race, is intimately connected with the history of the natural sciences. To take a comprehensive view of his subject, required a deep knowledge of history and philosophy; he had to peruse every work on these subjects that he might ascertain the origin of discoveries, a task of immense labour, and requiring a great degree of penetration, for many authors give only the germ of their ideas, and leave the facts almost as obscure as they are in nature.

The eminently philosophic mind of M. Cuvier is shewn in all its extent, when he gives, in a series of lectures, an analysis of the systems of ancient philosophers, and of the theories of modern philosophers. Admitting with the peripatetic school, and those of Bacon and Newton, that observation and experience are the only guides in the study of the sciences, he eagerly confutes the efforts which the metaphysical schools have made to connect natural phenomena with rational principles. He opposes himself with energy to the systems *à priori*, which, as he himself said of them, derive so few of their principles from things, and shew so well that they have been devised to explain the personal knowledge of their authors, and that facts insufficiently supported after the invention of a system, occupy as prominent a part in that system as those which are certain.

But there is another school, which, relying on facts, supposes itself sufficiently numerous to be rallied under a single principle, which it conceives itself to have discovered. This principle M. Cuvier has in like manner combated with all his power; but the question having been greatly distorted, we conceive it necessary to point out the error. For this purpose, we must give a sketch of the philosophical ideas which are to be gathered from all M. Cuvier's works.

As each organ performs a part in the economy of an organized being, and serves more or less in developing the phenomena which this being presents, he was of opinion that each being in like manner has a certain part assigned it in the economy of nature; that each was created for a definite purpose, and that it received from the Creator all the means and instruments necessary for fulfilling the end of its creation. He perceived in the structure of organs so well calculated to appropriate to the uses of individuals such of the physical properties of bodies as are necessary to their existence,—in this great assemblage of beings forming a chain of existences, which is perhaps as endless as space and time,—the design of a supreme intelligence, which it carries into effect by disposing all possible combinations of organs on different plans.

The partisans of the school of which we speak, without regarding the *intention* which appeared to M. Cuvier the most important consideration of all, since it is by it that we can appreciate the wisdom and infinite knowledge of the author of nature, saw in organized beings a unity of composition, in virtue of which they could only be modifications of a particular plan; or rather, for they do not agree on this point, a blending of a certain number of simple beings, to form others of a more complex and perfect kind.

This theory has been compared, inaccurately as we conceive, with the laws of Kepler and Newton, with the laws of mechanics and general physics which extend to all space, and which regulate not only our planetary system, but every body throughout the universe. If a comparison must be established between these laws and those which govern living bodies, it can be done with those only which are physiological. These are, in truth, general, and each may be applied to all the beings provided with organs in which it can be exercised. Beyond doubt, nutrition is performed in all animals by the same powers, and so with reproduction. In all animals with a nervous centre, nervous phenomena are produced in the same manner. So the laws which govern the functions, and which probably also regulate the animal kingdom, are general, and as immutable as the laws of physics, from which indeed they do not perhaps differ. In this sense there is undoubtedly a unity, and we are not aware that any one disputes it; but the question which is now before us is a question of the forms, or intention, which is of another character, but which also finds applications when referred to the planetary system, and the organic world. The laws of Kepler and Newton do not interfere with the fact, that the form of Saturn is different from that of other planets, and that certain of them have many satellites, whilst others have none, so that even for these dead bodies, there is not a unity of composition, a unity of plan. No more do the physiological laws of organized beings hinder their forms from varying according to their different destinies. But it is the form, and the organs, which reveal the destiny. The form then, has changed, the organs have been simplified, or complicated, when the end is different, when the intention requires it. The eagle and the serpent could not have the same form, and could not be provided with the same organs, and yet their physiological functions are performed by the same means,—they are the same. Moss and the oak vegetate by the same powers; their form alone differs. It is then of the form of organized beings, and not of their general functions, not of the general laws by which they grow, are nourished, propagated, feel, and, when animated, desire, that we here speak; but of the form, which, we repeat, in accordance with the end, with the part each being has to fulfil, cannot be every where the same, because this end and these functions vary. Moreover, to vary the form of beings, to arrive at the conditions of their existence, Nature has not only modified the organs with which she has endowed them, but she has given more to some, and granted fewer to others; she has displayed all the combinations of organs, which do not present physiological incompatibilities, all the combinations which are not contradictory; as, to vary the mode of action of physical forces, she has conferred many satellites on some planets, or provided Saturn with a ring. We may observe here, in passing, that if we remark so many varieties on the eleven planets of our system, what would it be could we carry our observations so far into the limits of space, as to discern those of all the other solar systems? Perhaps we might there find differences almost as remarkable, as among animated beings. We now know that these suns themselves, are not always single; that some of them are double, others triple, and consequently, there is not unity even there, where, without the telescope, the most daring imagination would not have supposed the contrary. Who can calculate the effect which two or three suns revolving round each other must produce on the planetary systems which belong to them? and yet, all these bodies, however varied they may be, obey the laws of Kepler and Newton, as all organized beings, however different may be their forms and functions, are obedient to physiological laws. We even believe, that it would not be too much to imagine, that these physiological laws extend beyond our planet: but it would be too much to suppose that the forms themselves which exist upon our globe, have representatives upon all the others.

Those who think that all the laws of nature are simple, are led to believe in the unity of composition, because they see simplicity in this law. It would not, however, be difficult to demonstrate, that it would be more simple to have many plans, than to torture any single one, as it would be necessary to do, to arrive at a general form from all the different ramifications. Without entering upon this discussion, we will say, that this idea of the simplicity of the laws of nature, which is so popular, without doubt arises from this, that all laws discovered by calculation, referring only to simple phenomena, are themselves simple; but that only proves that our methods are not yet sufficiently perfect to calculate phenomena that are complicated, and problems with many terms, and many that are unknown. Without doubt, the wisdom which the great Author of nature has shewn in all his works, does not allow us to doubt that He reaches his ends by the simplest possible of means: But these ways, simple to Him, may be so complicated to us, that they may reach far beyond the limits of our powers.

We shall conclude by remarking, that to prove the identity of the plan for all animals, the naturalists of whom we are speaking, are forced, in thought, to transpose the organs that annoy them, and to add or to subtract from the materials which compose them. According to this mode of proceeding, there can be no doubt that all animals may be brought to one solitary type: but it is precisely these transpositions, these additions and subtractions, to which they are obliged to have recourse, which prove that they are wrong; for these mental operations, which are made with so many *Ifs*, produce no real change in the being who is the subject of them; it does not the less exist with all the realities which constitute it a distinct being. Finally, when, after all these abstractions, the testimony of their senses still forces them to recognise the existence of discrepances, they have still a subterfuge;—they tell us, that these animals exhibit anomalies! Doubtless they do; but only with the laws of their own framing; and these anomalies are the best proofs of the inaccuracy of these laws. If the animal be regarded as nature presents it, and not such as it ought to be according to these rules, there will be no anomalies. These pretended anomalies evidently demonstrate that ideas very different from those of the theorist, have presided at the formation of organized beings, and that his are not the laws in virtue of which they exist. And that naturalist who recognises many general plans upon which all these living creatures may be arranged, is assuredly acting more philosophically than those who admit but one, and who are obliged, even after many abstractions, to allow that many of them present anomalies in one or many of their parts. From this statement of the question, we cannot see how it could be inferred from M. Cuvier's opposition to this system, that he was an enemy to the progress of science, and wished to repress the genius of those who sought to advance its interests. He was too much impressed with the maxim of Linnæus, that no human efforts can shake the truth, to attempt to oppose the propagation of a discovery.

The different doctrines which naturalists entertain, have, within these few years, been characterised by denominations taken from the prevailing political theories. It has been said, that in science there exists a *movement* and a *conservative* party; and Cuvier has been placed as a leader of the followers of the latter, because he would not admit as true some of the would-be philosophical systems of the day. We regard this as an extraordinary mistake. No one more than he was an advocate for the progress of the sciences, because to them he attributed the development of civilization, and from them, since their more rapid progress, he has dated a new era of our race, as may be easily learnt from the perusal of his remarks on science, embodied in his *Eloges*. No one more anxiously desired to see the natural sciences supported upon general principles similar to those on which the physical sciences are built. He who, in 1808, said to Napoleon, that "*general truths constituted the noble inheritance of our race*;"—he who, in the introduction of his work on fossil bones, remarked, "*Why may not natural history one day have its Newton?*" could not be the enemy of theory. But no one also was more capable than he to bring a matured judgment to bear upon those that have appeared, since no one like him had always present to his mind all the knowledge concerning organization that has been collected. "I have searched for them, I have myself found them," said he, speaking of universal theories, shortly before his death, "but I have not published them, because I have recognised that they were false, as I believe all have been that have yet been proposed. I say more, I say that in the actual state of science, it is impossible to discover one, and therefore it is that I observe, and that I commend observation, because it alone can lead to the discovery of a fact which will lead its discoverer to a true general theory." This fact," added he, "is perhaps of small importance in itself, but relative to the theory, it will become the principal fact, the key-stone of the arch. It is necessary then to seek for it; it is necessary to make science advance, but care must be taken that it does not advance the wrong way, as it has occasionally done, and as some perhaps are making it do at the present time: it is necessary to work, not with the design of supporting a theory, because then the pre-occupied mind only perceives that which favours it, but with the design of discovering the truth, because from the truth these theories will flow, and true philosophical principles, the truth in fact comprehending all philosophy.

We repel, then, with indignation, the reproach which some presumed to cast on him of being a mere collector of facts,—of rough materials which he could not use. Is a man to be regarded as the enemy of all theory, when he asserts of any given one, that it is false, and not in accordance with the facts? Is he to be regarded for ever unwilling to build, when he holds that the present materials are insufficient, that they cannot be held together without cement; that renewed efforts must be made, that each for a time must labour diligently at his post, and then a system will be reared.

Why may not he who had such a vast acquaintance with facts, if he could discover no general principle to explain them, why may he not confess this without being accused of stupidity? Are we to take offence at him, because the authors of the systems which he overturned, could not support them with satisfactory proofs; if, misled by their imaginations, they have followed amusing speculations rather than nature, or if their false logic has led them into erroneous conclusions? Are we to be offended at him, if all the systems of three thousand years have been overturned, and the observations alone have maintained their value? And may not this fact (for fact it is) be announced, but that he that announces it must be regarded as a man who is groping in the dark?

It is also not a little singular that such statements are made of a man who has been able to frame, with so much beauty, the only laws hitherto furnished by comparative anatomy, the laws of co-existence in organized beings, the laws of harmony between all the parts of a being, in virtue of which it can exercise the functions it has to fulfil in creation, and who has known how to apply it with so much success to living zoology for the classification of animals, and to the zoology of the dead for their resuscitation. It is especially in the results which may be deduced from a principle, in the fruitful developments it affords, that we can establish its reality; for if it be not susceptible of any application, if it be sterile, then it is false. Now, we demand of every honest naturalist what application he has made, or can make, of all the theories, anatomical, physiological, and metaphysical, of our days? But it is clear that the principle of the co-relation of forms applied to general zoology, has perfected its classification, and that, applied to subterranean zoology, it has given to the globe a history, and to zoology a foundation to rest upon. It is in vain, however, that we shall seek for the results of those systems that M. Cuvier has overturned; it is in vain we shall seek from them help in classifying, that we shall interrogate them in determining a fossil bone; we are not the less obliged independently to know that there are different forms for the mammiferæ and for birds, different for reptiles and for fishes; that the molluscæ and insects, and the zoophytes, have each forms that are peculiar to them; in short, we are obliged to act as if these systems had no existence; whilst with the principle of the corelation of forms, a single bone unveils to us all the animal, all the order, all the class. If we land on a foreign shore, the debris of organized bodies strewed over its beach will indicate the beings which inhabit it better than most of the inhabitants themselves; and if we penetrate into the bowels of the earth, the nature of the spoils which it conceals will reveal to us the nature of the earth better than the earth itself. And it is at the author of such a principle, one so rich in results, that the reproach of the neglect of all principle has been thrown!

His history of the sciences, his eloges,* analyses of the works of the Academy, and his reports on different memoirs, attest the truth of this assertion; but he knew how often theories prove fallacious, and that well established facts continue steadfast, and this he regarded as sufficient cause for circumspection. He by no means despised theories which shewed the genius of their authors; but he was slow in adopting them. He rejected the theory of the unity of composition, because it appeared to him contrary to facts; he conceived that it was founded on some facts of analogy more or less remote, and that the points of agreement had alone been regarded, and not the differences. We confess we have never been able to understand the reflections which some naturalists have cast upon Cuvier, that he never saw, in animated nature, any thing but differences, and that he did not seek for analogies,—inasmuch as all his labours had, as their constant aim, the natural classification of animals. But what is this arrangement, but an arrangement of beings grounded upon their analogy? It is the analogies which lead to the union of the species, that prove these genera may be formed; it is the analogies which groups the genera into a family, the families into an order, &c. &c. But besides these real analogies, living beings present differences which determine the divisions, and it is only for the sake of abridgment, that there may not be perpetual repetitions, that these analogies, once admitted, it is then only necessary to note the differences. His method appears to us most philosophical, and the only one capable of reaching the end proposed, viz. the knowledge of the species, without our being obliged to write volumes on each of them; and we shall cite as an example of his mode of procedure the last of his zoological works, his *Histoire des Poisons.* It is only after having studied, and analyzed, and compared all the species, that he has formed his genera. He takes in each genus one of the species for a model, and he describes it with care; then to each of the following species he devotes only a few words to note the differences which distinguish it from the first, and from all the others;—but it is not to be forgotten that all the relations, all the analogies, all the resemblances with the model species, are tacitly comprehended in the position assigned to it, with the exception of the differences which constitute it a distinct species. His method is the same in comparative anatomy;—he describes the organs of man, taken as a point of comparison, and then, by making an abstraction of the resemblances, he has only to note the differences; but the parts which he compares are, by the fact of this comparison, considered as analogous. In his fossils he follows the same course. Having demonstrated in detail the osteology of living crocodiles, he does nothing more for the fossil crocodiles than to exhibit the differences which distinguish them from the living crocodiles, and those which they present amongst themselves.

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* *Vide* note at end of this memoir.

It is true that, with some other naturalists, M. Cuvier does not perceive analogy of form, and of the relative position of organs, and of connections between every animal,—and here is the precise subject of the discussion which now engages us. He cannot admit that the lungs or the branchiæ of vertebral animals, for example, are in the same connections as the branchiæ of the mollusca and the crustacea, situated with the one on the sole of the foot, or fixed on their feet themselves,—and with others often on the back, or round the anus. He does not admit the analogy between the skeleton of the vertebral animals and the skin of the articulata; he cannot conceive that the *tænia* and the *octopus* were constructed on the same plan,—that there is a unity of composition between the bird and the *echinus*,—between the whale and the *colimaçon*, in spite of all the art by which their differences are attempted by degrees to be effaced; and we own that we think as he does, and that we do not believe that there is any thing common betwixt these beings but *animality*,—that is to say, the execution of the general functions of life.

It is this difference of connections, or of the relative position of organs, that prevents M. Cuvier from admitting the theory of the progress of the fœtus through all the links of the animal scale, inferior to its own species; since it is evident that in a complete whole, where all the parts are linked to one another, as in an animal, the organs cannot change their place and form, so as to correspond one day to a zoophite, and another to an insect,—afterwards to one of the molluscæ, then to a fish, a reptile, a bird, and finally one of the mammiferæ. Add to this, that certain of the lower animals have some of their organs more complicated than those of animals of a higher grade. For example, the stomach of the carnivoræ is much more simple than that of the ruminantiæ; and hence it happens that this viscus, at first simple, when it resembles that of the fish and certain reptiles, becomes complicated to arrive at the development which exists in the ruminantiæ, and that it again simplifies itself, to assume the form which corresponds to the carnivoræ,—to quadrupeds, and to man. A crowd of physiological impossibilities could in this way be cited, for there are such, as well as mathematical impossibilities; and this consideration alone, viz. that the organs do not all follow the same scale of increase or decrease,—that there are some that augment, whilst others diminish, or altogether disappear, appears to us to present an absolute impossibility to this progress of the fœtus through all the links of the scale,—a theory which we believe to be founded only on a deception of apparent form, either of the head or the extremities, when these parts are in a state of incipient development. He rejected it especially, from the conviction that it would be attended, like all other false systems, with very injurious consequences. While he gratefully acknowledged that some discoveries are due to theories, whose authors, in seeking what they hoped to prove, encountered what they did not seek, he still regarded them as injurious, inasmuch as they flattered that natural tendency of the mind to make science repose on opinions already formed, and dispense with laborious study. It was, therefore, from a love to science, from a devotion to the discovery of truth, and not from personal motives, or the vain desire of imposing his opinions on others, that he opposed these doctrines; and it was even with the design of bringing back such minds as these to what he regarded the true principles of science, that he had determined, in the last years of his life, to resume his lectures in the College of France.

Considered in a progressive point of view, the theories seemed to him far from answering to the idea which their authors form of them. He regarded them only as modifications of the ancient pantheistic system, which has already made its appearance in the world under many different forms.

At the time of his death, he was labouring, not only at his History of the Sciences, but at a great descriptive work on natural history, the History of Fishes. His object was to shew, by example joined to precept, in what manner we ought to seek a knowledge of species—that definite end of natural history, and how to group these species into genera, and associate genera into families, and families into orders.

The desire of knowing these animals, that he might be able to draw up the work which was to crown his labours, a work which was continually in his mind, and for which he had amassed a great number of drawings, partly executed by his own hand, and which he called his great Comparative Anatomy, was the circumstance that dictated his choice. A decided predilection, produced, no doubt, by his first studies of marine animals, attracted him strongly to ichthyology, so that, among the instructions he gave to travellers, he never forgot to draw their attention to fishes.

We ought to mention on this occasion, that the ascendency of his genius had given him such an authority, that in every country, as was said of Linnæus, nature was interrogated in his name. Every young naturalist looked upon his recommendations as orders emanating from Science herself, and calling upon him to procure the objects of his desire, that they might be consecrated to him, as the singular productions of foreign countries were anciently presented to the divinities. In this way the expeditions undertaken by order of Government, as well as those of a more private nature, seemed to have been projected expressly for him. Unanimously acknowledged as the legislator of science, either the objects themselves, or faithful representations of them, were submitted to his examination. Owing to this, the collections in the museum in general are prodigiously augmented, and the collection of fishes in particular, has been doubled through his means.

Besides these larger works, M. Cuvier has published numerous memoirs upon particular points of anatomy,—upon the head of vertebral animals,—the eggs of mammiferæ, and on those of some of the mollusca. He has given a description of some new species, and has furnished many articles to the *Dictionnaire des Sciences Naturelles*, and especially its prospectus, in which there is a rapid exposition of his method of philosophizing, in relation to natural history.*

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* The following eloges by Cuvier are published in this Journal.

Edinburgh Philosophical Journal.

Werner, vol. iv. p. 1.

Edinburgh New Philosophical Journal.

Sir Joseph Banks, p. 1; Bonnet and Saussure, p. 213, vol. ii. Adanson, p. 1; Priestley, p. 209, vol. iii. Herschel, p. 1; Pallas, p. 213, vol. iv. Daubenton, p. 1; Cavendish, p. 209, vol v. Beauvois, p. 1; Haüy, p. 205, vol. vi. Pinel, p. 205; Bosc, p. 274, vol. vii. Halle, p. 1; Corvisart, p. 9; Rumford, p. 209, vol. viii. Claude L. Richard, p. 201, vol. ix. Duhamel, p. 1, vol. x. Davy, p. 1; Vauquelin, p. 209, vol. xv.

SOME REMARKS ON THE PLANT WHICH YIELDS THE CASCARILLA BARK.

By DAVID DON, Esq. Libr. L. S., &c. Communicated by the Author.

There is reason to believe that many species of *Croton* afford a bark partaking more or less of the properties of cascarilla, and indeed this opinion is borne out by analogy with other genera among whose members similar qualities are generally found to prevail. It is a curious fact, however, that the *Croton cascarilla* of Linnæus possesses none of the sensible properties of cascarilla bark. The late Dr Wright, whose knowledge of the medicinal plants of Jamaica was unrivalled, appears to have been the first to determine this fact, and that the bark in question was derived from the *Croton Eluteria*, of which a faithful representation will be found in Sloane's Jamaica (vol. ii. t. 174. f. 2), referred incorrectly by Linnæus to his *Croton glabellum*. The same opinion seems also to have been entertained by Linnæus himself, for in the first edition of his *Materia Medica*, the *Cascarillæ cortex* is mentioned as one of the products of *Clutia Eluteria*, but he afterwards, as now appears on very insufficient grounds, altered his opinion in favour of a plant with which he was entirely unacquainted, except from the figure in Catesby's Carolina (vol. ii. t. 46). Of this plant, which he named *Clutia cascarilla*, he had then seen no specimen, and in the *Species Plantarum*, where it occurs for the first time, he has stamped it with the usual mark of an obscure species. Of *Clutia Eluteria* he had a sample, from which he evidently drew up his description, in the *Amœnitates Academicæ*, although he confounded with it a Ceylonese plant, which he had taken up in the *Flora Zeylanica* from Hermann, and likewise two other totally different species, the first figured by Plukenet, which is *Croton micans* of Swartz, and the second by Seba (*Thesaurus*, vol. i. t. 35. f. 3). In the Lambertian Herbarium, there is a specimen from Curaçao exactly resembling the last-mentioned figure, which I should be inclined to refer to *Croton nitens* of Swartz. The specific character, which occurs throughout all Linnæus' works, of *Clutia*, or rather *Croton Eluteria*, appears to refer entirely to the Ceylon plant, whose history is still involved in great obscurity.

Dr Wright considered the Eleutheria and Cascarilla barks as the produce of *Croton Eluteria*, and this opinion is now pretty generally adopted by pharmaceutical writers; but I am disposed to regard them as derived from two distinct species, and I rather incline to the opinion of Boulduc, Spielmann, and others, that the cascarilla bark is a production of the Spanish Main, for it does not appear that it ever was obtained from Jamaica, or even from the Bahama Islands (from one of which the appellation Eluteria or Eleutheria is derived); and it is now ascertained, from the recent observations of Messieurs Schiede and Deppe, that a bark, agreeing in every particular with the cascarilla bark of the shops, is collected extensively in the vicinity of Jalapa, at Actopan, and in the district of Plandel Rio, in the province of Vera Cruz, Mexico, where it is known by the names of Copalche or Quina Blanca. These gentlemen considered the plant at the time to be identical with the *Croton Eluteria*, but although closely related, it is nevertheless essentially distinct from that species, differing in its broadly cordate, 5-nerved leaves, which are slightly peltate at their insertion, and of a more coriaceous texture. In *Croton Eluteria* the leaves are ovate-oblong or elliptical, furnished with a solitary midrib, having obliquely transverse ramifications, and the base either obtuse or somewhat attenuated, but neither cordate nor peltate. The inflorescence is racemose, and in other respects nearly similar in both species. The tree grows to the height of 25 or 30 feet, is much branched, and clothed with a profusion of broadly cordate leaves, silvery underneath, and numerous clusters of white flowers. The bark is exteriorly of a grey colour, pale brown within, of an even fracture, possessing a strong aromatic flavour, and an agreeable bitter taste, and in other respects accords with the Cascarilla bark of the shops, for I have carefully compared samples of the bark sent by Messieurs Schiede and Deppe, with others from the Apothecaries' Hall, and I think there cannot be a question as to their identity. To the Mexican species I would recommend the application of the name of *Croton Cascarilla*, that of *Pseudo-China* given to it by Professor Schlechtendal, in his recent treatise on the subject, being in many respects objectionable, and leaving to the *Croton Cascarilla* of Linnæus the more recent epithet of *linearis*, applied to it by

Jacquin, being perfectly convinced of the identity of the latter with the Linnean *Cascarilla*, and that the distinctions hitherto relied on to keep them apart are of too trivial and variable a nature to be entitled to the importance which has been assigned them. The specimen in the Linnean Herbarium appears to have been communicated by Philip Miller, and belongs to the West India variety, with narrower leaves, and consequently is what Jacquin meant by his *Croton linearis*. The glands at the insertion of the leaf, I observe, vary from two to four, although, in the specific character of *linearis*, they are stated to be uniformly two, and three in *Croton Cascarilla*.

REMARKS ON MR NICOL'S OBSERVATIONS ON THE STRUCTURE OF RECENT AND FOSSIL CONIFERÆ. *By* WILLIAM MACGILLIVRAY, *A. M. F. R. S. E. &c.*

In a paper entitled "Observations on the Structure of Recent and Fossil Coniferæ," published in the last Number of the Philosophical Journal, Mr Nicol, alluding to Mr Witham's observations on the structure of certain fossil plants, finds it "necessary to guard the scientific world against placing too much reliance on a work, containing so many inaccuracies." As I had the pleasure of assisting Mr Witham in his investigations, I believe it is in some measure necessary that I should say a few words regarding Mr Nicol's statements. As to the observations on the structure of the recent and fossil plants in question, there is no occasion of adverting to them, as geological botanists can easily satisfy themselves respecting the accuracy of the descriptions and figures that have been presented by Mr Nicol or Mr Witham.

In the first place, it is asserted that "several distinct fossil genera have been indicated by a person who has examined, and that too very superficially, only three slices of three recent pines, differing not essentially from one another." Who is the person? For myself, I would say that the assertion is not true, were its rashness not evident to all. Certain, however, it is, that the structure of recent Coniferæ has yet been but very superficially examined; nor is even Mr Nicol's explanation of it nearly so satisfactory as that which was given long before any man in Scotland had thought on the subject.

Secondly, Mr Nicol states, that to him is owing the discovery "that all the fossils retaining the ligneous structure in the coal and lias formations, are of coniferous origin, and that, with one exception, those of the tertiary formations are either monocotyledons or dicotyledons." I am not aware that Mr Nicol ever pointed out such a discovery to me, nor do I believe that Mr Witham was apprised of it by him. That the ligneous fossils of the carboniferous series and lias, of which sections were exhibited to me by both these gentlemen, appeared to be coniferæ, was what any person examining them might readily perceive; and neither Mr Witham, who directed, nor myself, who represented, could be ignorant of the fact, which was stated before Mr Nicol's discovery was heard of. When I began to arrange and draw the slices which afforded subjects for the plates of Mr Witham's first work, Mr Nicol was aware that coniferæ present a different *pattern*, so to speak, in their transverse sections from dicotyledonous or monocotyledonous trees: but this was to me evidently the whole extent of his knowledge, nor was he in the least aware of the nature of the different apertures forming the lacework of these sections.

Thirdly, Mr Nicol, speaking of a certain fossil, says, "were this slice divided into two unequal portions, one of them would have been a Peuce, and the other a Pitus or Pinites; and parts would have furnished materials for the genus Anabathra of Witham." This is to a certain extent very possible, and in Mr Witham's work, it is actually stated that, "in fact a single trunk often presents appearances characteristic of the two genera, according to the state of its parts. Thus a Pinites regularly articulated in its unaltered and straight cellules, becomes in some degree a Peuce, when the cellules are curved and distorted." But the structure of Anabathra is so different, that with respect to it the remark does not hold good. As to the proposed genera Pitus, Peuce, and Pinites, it is distinctly stated, that they run into each other, and that connecting links may occur: nor are they presented otherwise than for the purpose of exciting observers to investigate the subject more fully. As yet, however, nothing has occurred that can tend to invalidate the generic and specific characters that have been elicited.

Fourthly, "The method of investigating the structure of coniferæ by the characters displayed in the longitudinal sections has" "*not* been much vaunted as a new discovery," in Mr Witham's second edition; nor was it Professor Lindley who first published the advantages resulting from it, although he was the first who applied it to the fossil coniferæ; but whether Mr Nicol has often employed it or not, he is well aware that at the period of the publication of Mr Witham's first work, he held it in utter contempt, alleging that longitudinal slices shew nothing whatever. In consequence of representations of this kind, a single longitudinal section was all that I was allowed to introduce among the drawings, although I represented to Mr Witham the necessity of examining the tissue in various directions. Notwithstanding this radical defect, Mr Witham's work answered effectually the very important purpose of opening up a new field for investigation.

Fifthly, As to the slicing and polishing ascribed "by unwise friends" to Mr Witham, I am sure that gentleman will leave Mr Nicol and the lapidaries to settle the matter as they may think fit.

Sixthly, The comparative accuracy of the descriptions and figures in Mr Witham's book and Mr Nicol's paper, will necessarily be decided upon by those who find the subject sufficiently interesting to engage their attention. It must of course be admitted, that Mr Witham was the first who drew the attention of geologists to the subject of the internal structure of fossil plants as disclosed by transparent slices, similar to those made in recent plants for the same purpose, and few will be disposed to deny that his labours have been productive of benefit to science. The very attack that has been made upon him is a proof of his great merit; for almost every remarkable discovery has been similarly treated. On this subject, however, I do not intend to expatiate, it being merely my duty to assure "the scientific world," that my drawings in Mr Witham's work were carefully executed, and that the descriptions in which I assisted were certainly not intended to deceive. Although Mr Nicol *now* chooses to detract from their merits, he expressed his entire approbation of them when Mr Witham's first work was finished; and assuredly those of the second are in no degree inferior.

Lastly, Should any further remarks be offered on the subject, I shall have an opportunity of representing matters in what seems to me the true light in which they ought to be viewed; more especially if Mr Witham should not consider it necessary to take up the subject. I shall also have the pleasure of presenting a historical view of the progress that has been made in the examination of the recent coniferæ, and, *if possible*, of adding my mite to the general store.

In the mean time, although I have been obliged to speak for myself, I am not disposed to claim the slightest merit for a single fact of those published by Mr Witham. That gentleman, having at great labour and expense collected numerous specimens of fossil trees, employed a lapidary to slice them, and examined them with intense interest, brought the more characteristic slices to me, explained the objects which he had in view, and directed me to execute the drawings, and arrange the materials which he had prepared for the work. Mr Nicol having also made a collection of slices, gave me permission to select such as I might find useful. Some of these were accordingly employed, and, as I have said, of the manner in which they were represented and described that gentleman repeatedly expressed his approbation. During the progress of the second work, all communication with Mr Nicol ceased.

ON A METHOD OF SO FAR INCREASING THE DIVERGENCY OF THE TWO RAYS IN CALCAREOUS SPAR, THAT ONLY ONE IMAGE MAY BE SEEN AT A TIME.*

UNDER this title, Mr Nicol some years ago described in the Edinburgh Philosophical Journal, (No. XI. p. 83.) an instrument which, at first sight, does not seem to possess any superiority over a good achromatised prism of calcareous spar, but which, however, I am convinced from my own experience, does possess important advantages, inasmuch as it throws the images farther asunder than any one I have met with; and I do not even except the parallelopiped, prepared according to Wollaston's directions from two prisms of calcareous spar in which the chief sections are made to cross at right angles.* Nicol's instrument not only answers in place of tourmaline, the green variety of which, as is well known, is difficult to be obtained; but it actually surpasses it essentially, in a property so important in many investigations, that of exhibiting the colour phenomena of crystals and other bodies in polarised light in a manner perfectly pure, and free from foreign tints of colour.

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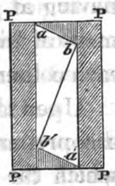
* From Poggendorf's Annalen der Physik und Chemie, vol. xxxix. p. 182.

The construction of this instrument will be most easily understood, by reference to the accompanying figure, which represents the instrument as sent from England, and for the use of which I have to thank the politeness of Professor Dove. A section of it is here given of the natural size. *ab*, *a′b′*, is the principal section of a rhomboid of calcareous spar, which has received the dimensions here represented by cleavage. The long edges, *a b′* and *b a′* are the natural obtuse edges of the rhomboid; while the terminal planes *a b* and *a′b′*, are so cut as to form an angle of 68° at *a* and *a′*; whereas, in their natural state, the angle is 71°. The rhomboid thus modified, is cut through in the direction of the line *b b′*, perpendicular to the principal section and to the terminal planes *a b* and *a′b′*; and after both the new surfaces have been polished, they are again united by Canada balsam. Both the prismatic halves of the rhomboid† can also, as is more easily effected, be cut in the necessary form from two different pieces, taken either from one or from two crystals. The whole is inserted in the cork PPPP, which has been cut through and again united.



If we look at an object through this rhomboid placed longitudinally, parallel to the edges *ab′* and *ba′*, we see only one image in this direction, and that the ordinary one; the extraordinary image comes first into view when we incline very much the long axis of the instrument in relation to the line of sight, and when the power of seeing directly through is not at all disturbed. In this manner the field of sight possesses dimensions which are sufficiently great for optical experiments.

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* A similar construction for quartz, described and figured in Herschel's Treatise on Light.

† Accurately speaking, the combination is not a rhomboid, but an oblique rhombic prism, since the planes make unequal angles at the three edges of the obtuse solid angles.

Mr Nicol has not expressed any opinion as to the cause of this great divergence. It is clear, however, that the chief cause is to be sought in the action of the Canada balsam, which, owing to its refractive power (1,549), and which is that between the ordinary (1,6543), and the extraordinary (1,4833), refraction of calcareous spar, will change the direction of both the rays in an opposite manner before they enter the posterior prismatic half of the combination. Without this action of the Canada balsam, the cutting through and uniting the rhomboid would be of no use, as was at first evident to us, but which has been confirmed by experiment.* Whoever will take the trouble, may calculate accurately the course of both rays, and even of each colour, by means of the already existing data and formulæ.

Upon the plan just described, the mechanician Hirschmann has prepared several of Nicol's rhomboids, which are in all respects the same with that sent from England. Two of them which I myself possess, are as perfect as I could wish them to be. In looking through one of them at a fine line drawn on paper, the line appears quite simple.† If we use both, and place them behind one another, directing them upon an object in a horizontal direction, we find, that, when the principal sections are parallel, the object is seen with nearly as much brightness and absence of colour, and with nearly half the distinctness, as when it is regarded with the naked eye; if, however, we turn one round until the principal sections are made perpendicular to each other, we then have, at least in the middle of the field, perfect darkness. The weak light which is still visible,* is evidently derived from reflection on the lateral planes, and would undoubtedly be removed if we blackened them. If, while the principal sections cross one another, we insert between the two rhomboids a plate of calcareous spar which has been cut perpendicular to the axis, we obtain a combination entirely formed of calcareous spar, which exhibits the phenomena of coloured rings with the black cross as distinctly as they are obtained by any other method. If we now interpose a plate of mica, of the proper thinness, before and behind the plate of calcareous spar, between it and the rhomboid, so that the axes of these two little plates may cross each other at right angles, and at the same time bisect the right angle between the two principal sections, we then see the coloured rings without the cross having the black centre, and quite similar to the Newtonian rings. An interesting modification of these phenomena has been described by Mr Airy, and was, at an early period, exhibited to me by Professor Nörrenberg.

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* Mr Nicol was aware, that the increased separation of the images depended on the Canada balsam with which the halves of the rhombic prism was united. If the two parts be united by water, the light sustains total reflection.—*Edit.*

† On the other hand, the extraordinary image which is obtained by the strong inclination of the rhomboid receives a mixture of the ordinary one, and the field appears on that account brighter. This is of no disadvantage in the use of the instrument. We also observe a series of feebly coloured edges on the boundary of the two fields of sight, and this is also the case in the rhomboids made in England.

It may be remarked, that Brewster had previously employed an analogous method to destroy one of the images in double refracting crystals.† For this purpose, he cuts a prism from a crystal of this nature, (as, for example, calcareous spar, arragonite, saltpetre, carbonate of potash, &c.) and in such a manner, that its angles may be parallel to the optical axis—he leaves two of the planes of the prism rough, and fixes glass plates upon it by means of a suitable substance. If the substance has the same power of refraction as the ordinary ray of the crystalline prism, this ray goes through, since, in relation to it, the planes of the prism are equally polished; but the extraordinary ray becomes scattered on all sides as on a rough plane, and is thus in some measure interrupted. If the substance has the same refractive power as the extraordinary image, the reverse takes place. For example, if we fix the glass plates on a prism of saltpetre, by means of copal balsam, or what is still better, cassia oil, the ordinary ray, whose refraction is 1,511, passes through unweakened, while the other disappears, or appears only as a red light. If, on the other hand, we take for the uniting substance, alcohol, or white of eggs, the extraordinary ray, where refraction is 1,328, passes through, and the ordinary ray is lost. This plan is decidedly inferior to that of Mr Nicol, as the images it affords are dull and coloured.

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* By making the lateral planes rough, which may be done with a file, there is scarcely any lateral reflection.—*Edit.*

† Phil. Trans. for 1819, p. 146, and Edin. Encyclop. art. Optics, p. 600

ON PETROLEUM OR MINERAL OIL. *By Dr Reichenbach.*

Although petroleum has been found in nearly all the countries of the globe, from the finest Persian naphtha, through all the gradations of the white, reddish-brown, and black mineral oil, to the impure earthy pitch, yet we have remained quite in darkness as to its origin, notwithstanding the numerous opportunities presented for the investigation. Among the many conjectures proposed by various naturalists, that theory has met with most support which ascribes the formation of petroleum to subterranean processes of charring and combustion which had previously been in operation in coal deposits. This view however is entirely hypothetical, for the formation of petroleum has never been observed as a consequence of combustion in coal-mines, nor has any one succeeded in obtaining it by the carbonisation of coal either in open or in close vessels. The consideration that this subject stands in the closest relation to the products of the dry distillation of organic bodies, has induced me to make experiments upon it, and the following notice will shew how far I have succeeded in removing the veil which has hitherto obscured the question.

In a large iron retort, I placed 50 kilograms of coarsely divided coal, and mixed it with a considerable quantity of water. The coal was from Oslawann, two miles west from Brünn, from the principal coal formation, and is one in which species of Calamites, Sphenopteris, and Odentoperis are found in abundance. I now carried on distillation so long as water passed over, but not longer, so that no carbonisation could take place. It was merely with the intention of avoiding this with certainty, and to prevent all deception, that water was employed. As soon as the products of distillation began to pass over, I perceived a coating of oil on the water, and upon opening the apparatus the smell of petroleum became at once distinctly perceptible. I repeated this process eight times with fifty kilograms of coal, and when I had united the fluids obtained, separated the oil and rectified it without adding any mixture whatever, I found that the quantity of oil amounted to 150 kilograms. This proportion would give one loth of oily fluid to the Austrian cwt. of coal.

As the amount obtained in this manner was so small, I caused a vessel to be filled in the coal-pit with newly quarried coal, had it well closed and brought to the surface, and immediately subjected its contents to the process of distillation. The oil it afforded was decidedly more abundant, but it did not exceed the double of that obtained from the usual coal exposed for sale in magazines.

Upon more minute examination, the oil exhibited the following properties.

It was perfectly clear and transparent, had a light greenish-yellow tint of colour, which would probably have disappeared on second distillation; it was extremely fluid, and had completely the smell of a tolerable pure natural naphtha. The taste was the same as that of white petroleum. The specific gravity at a temperature of 20° Cent. was 0.836, therefore agreeing in this respect with the petroleum of Amiano as described by Saussure. In the open air it evaporated pretty rapidly, and its boiling point was 167° C.; thus nearly that of the Persian naphtha, which Thomson gives as 160° C.

Light and air, or the rays of the sun, produced in the oil no perceptible alteration. It could, however, be inflamed without the use of a wick, and could be made to burn upon the whole surface, giving at the same time a strong light, and having the same thick sooty smoke as petroleum.

Upon placing the oil and the ordinary petroleum of commerce under bell-shaped glasses, each separate and mixed with iodine, both attracted iodine vapours from the air and became brownish-red. On the other hand, the iodine took the oil vapours from the air, and became blended with it. The oils after some time became thick, and then clear again at the same time after the deposition of a small portion of a dark iodine combination.

Powdered sulphur was dissolved in certain quantities by cold oil as by petroleum; when heated, the quantity dissolved was much greater, and on being again cooled crystallized naphtha was obtained.

When potassium was placed in the oil, some bubbles were evolved in the first moment, as in purified mineral oils; soon, however, these entirely ceased, and the metal could be preserved without any difficulty, and completely protected from oxidation. In a certain time, as is the case under similar circumstances in the mineral oil, yellow reddish-brown flakes appeared at the bottom of the vessel.

Shaken cold with concentrated solution of potash of sp. gr. 1,36, no solution took place; but after being allowed to stand for a short time, yellowish-red portions of matter were found, which were precisely similar to those afforded by petroleum, and which seemed to be a similar combination which swims on the surface of the potash solution.

Smoking *sulphuric acid* is by slight heating rendered brown by both oils, but remains clear; and both the oils remain colourless. When shaken with English sulphuric acid of 1.450, the oils are separated into two parts, of which the one is brown and the other clear and colourless. The last smells like mineral-oil, and burns with a strong sooty smoke.

White *nitric acid* of 1.850 dissolves both in the cold, but becomes reddish itself, then clear, and afterwards separates slowly a small quantity of a brownish substance.

Water dissolves neither, but acquires the same smell from both.

Alcohol dissolves both in every proportion. Spirit of wine of 0.84 has the same dissolving powers in regard to both, viz. as 9 : 1.

Ether dissolves both in unlimited quantity. Water contained in it is expelled by both.

Almond oil can easily be mixed with both.

Camphor is easily soluble in both.

Sandaric is slowly dissolved by both.

Mastic becomes opaque on its surface in both; by the application of heat it is dissolved by both, leaving a white opaque residue.

Caoutchouc swells rapidly in both, without, however, being dissolved in the cold.

As it thus appears, that not a single distinct reaction, and also not the slightest difference, is exhibited by these two oils, I regard it as superfluous to pursue farther the comparison between two substances, against whose complete identity not a proof presents itself. According to this investigation, I consider myself entitled to draw the conclusion, *that the newly discovered oil is not a new substance, but in fact nothing else than a real and true petroleum.*

A confirmation of this might perhaps be brought from an elementary analysis, and I shall not omit to effect this; but, since petroleum, taken as a species, is subdivided into a whole series of kinds, from the naphtha through all the varieties of mineral-oil to earthy pitch, and, since these are not simple, but are mixtures of various substances, and in completely different proportions; it is evident that, until the component parts have been obtained, an elementary analysis can afford no constant, and indeed no diagnostic period. It can give nothing but an approximation which will inform us in what part of the series of petroleums the new oil ought to be placed.

I endeavoured to extend similar observations to other descriptions of coal. For this purpose I operated in the same manner on several cwt. of brown coal from the quader-sandstone formation of this neighbourhood. But I obtained no mineral oil; and the water of distillation did not even possess the smell of petroleum.

From this examination it appears, that the hitherto adopted conjecture, that petroleum is the product of the action of a higher temperature on combustible minerals, is not correct, and must be abandoned. It is rather to be regarded as *pre-existent in coal, and as one of its component parts.* The question as to its existence in a state of chemical union cannot, it is true, be decided; but that such is the case is improbable, as the greater part of the oil disappears when coal is exposed to the air. I am rather inclined to believe, that it is merely mixed with the other substances, and that, being very finely divided, it is retained partly by adhesion. In order to expel it entirely, it is necessary to heat the coal to the boiling point, that is to 167° C., which I intentionally did not try, and which I advise those who wish to repeat any experiments not to attempt, as, when the water has entirely vanished, it is nearly impossible to heat the coal equally, or to avoid having the outer portions aggregated together and exposed to a heat which goes on increasing, and which then causes the formation of the first products of dry distillation, and in this manner gives rise to an entirely false result. That matter, which passes over with the watery vapour, is, it is true, only the product of the tension of petroleum at 100° C.; but one can be assured by this procedure, that only watery, and no dry distillation's products are obtained. The coal which remains loses nothing of its external characters except the lustre of its fracture.

If we wish to proceed further, and to inquire how petroleum has been formed, and whence it derives its origin in coals, I believe I may venture to attempt a reply. In all my earlier labours with mineral oils, I never trusted to the purity of the substances obtained by purchase, as it always appeared to me that the mineral oil sold had the smell of turpentine, and that it was rendered impure by it. The impossibility of making mineral-oil free from soot, in which peculiarity it is so remarkably distinguished from Eupion, strengthened my mistrust. I was not a little surprised to find the same turpentine smell in that mineral which I myself had prepared, and of whose purity I was convinced. This odour was particularly distinct when I rubbed some drops between my hands. I perceived that the turpentine odour belonged in fact, up to a certain point, to the finest petroleum, and that it was the less mixed with other odours the purer the substance was. Upon comparing further the physical and chemical properties of natural and my own artificial petroleum with those of turpentine-oil, I obtained the following results.

In *transparency*, *absence of colour*, tint of the accidental tinge of yellow colour, and in *fluidity*, they are precisely alike. The smell seems to approach in a remarkable manner, and is merely altered by various accidental mixtures; as turpentine-oils from different pines smell somewhat differently, so do the petroleums from various sources vary in the same manner: all have, however, a more or less distinct turpentine-oil smell, which is most distinct when the substance is rubbed between the hands. In *taste* the artificial mineral oil and the rectified turpentine-oil approach very nearly: that of the latter is stronger, but of the same description. The *specific gravity* of turpentine-oil is given in elementary works, as from 0.79 to 0.87; this gives a mean of 0.83 which is nearly the specific gravity of the mineral-oil from

Amiano; and that made artificially. The *boiling point* of turpentine-oil is from 158° to 160° C., that of Persian naphtha according to Thomson is 160°, and that of artificial petroleum is 167° C.; variations which in mixed substances can hardly be taken into account. During distillation the boiling point of both becomes higher, owing to the partial separation of the component parts. All are *volatile* in the air in nearly the same degree if they are rectified. They make a greasy mark on paper which afterwards disappears. All these oils are distinguished by the *soot* formed when they are in combustion. They all dissolve *sulphur* in a similar manner. The property of turpentine oil observed by Davy of extracting iodine from *iodine water*, is also possessed in a most complete manner by petroleum. Not one of them is soluble in *water*, but all give it the peculiar odour. When mixed with concentrated *sulphuric acid*, they acquire a brown tint; but in all of them a colourless portion is separated when the mixture is not disturbed. *Potassium* disengages in all some bubbles, then turns tranquil, remains metallic, and the brownish-yellow matter is formed. The last mentioned effect, and the disengagement of the bubbles, are more distinct in turpentine-oil. In *spirit of wine* they all shew the same solubility. *Caoutchouc* swells out in a remarkable manner in all, but is not dissolved in the cold. All three appear to be compounds, or probably mixtures, of several substances, which may to a certain extent be separated by distillation. Hence arise the discrepancies in the analyses of Saussure, Thomson, Opperman, &c. The oxygen, as one of the ultimate component parts, is awanting in all of them, or at least in the older analyses. The opposite opinions of Saussure and Opperman upon this point are probably both correct, but each for a distinct substance. But here it is sufficient to know that there are certain turpentine oils which are quite as free as petroleum from oxygen.

This comparison of their most important relations is sufficient to point out the resemblance of these oils, and to give room for the conjecture that their principal component parts are identical. When we remember that coal is so filled with remains of plants that its origin has been attributed entirely to the destroyed vegetables of an early period, it must appear probable that petroleum was formed from such plants as afforded these oils, and in one word, *that our mineral-oil is nothing but the turpen-*

tine oil of the pines of former ages; not only the wood, but also large accumulations of the needle-like leaves of the pines may also have contributed in this process. We should thus have the satisfaction of obtaining, after the lapse of thousands of years, information as to the more intimate composition of those ancient destroyed structures of the period of the great coal formation, whose comparison with the vegetation of our globe is the subject of so many investigations; and we should thus be able to rank the substance with the few which from a later period, viz. that of the quadersandstone formation, have reached us in the form of amber, and a few other similar bodies. The appearance, then, of petroleum in many of the scattered springs of the earth does not depend on combustion, but is, as I believe, simply the result of the action of subterranean heat. According to the information we now possess, it appears that it is not necessary that strata should be very deep under the surface, in order to be reached by a heat equal to the boiling point of water or mineral-oil. In such a position the oil must have suffered a slow kind of distillation, and under appropriate circumstances must gradually have found its way to the surface, or must have impregnated so completely a portion of the earth, as to enable us to collect it from wells, as in various parts of Persia and India.

In my memoir on Eupion, in this Journal (for 1831, vol. ii. part 2), I have mentioned the possibility of the existence of that substance in mineral-oil, although I could not succeed in detecting any correspondence between these bodies or their component parts. In that essay, I omitted mentioning many experiments performed with that view. From the explanations afforded by the present investigation, it is now evident why my efforts to identify these two substances proved unavailing; substances which, according to the hitherto adopted opinion as to mineral-oil, I regarded as of similar origin, or as formed from dry distillation, whereas now an entirely different view as to mineral oils presents itself. The Eupion is a product of dry distillation; whereas the mineral-oil is derived directly from the action of vegetable life, and both substances, although they exhibit external points of analogy, are yet widely different in their nature and probably in composition. Indeed, one might, on the contrary, rather look for mineral-oil in Eupion, when the tar from which the Eupion has been obtained is coal-tar. For, in the carbonization of coal, its mineral-oil will be the first to disappear, and then to mingle itself with the Eupion of the tar. In the subsequent rectification of the tar, the Eupion, the mineral-oil, and the remaining volatile parts will pass over first, and become mixed. As both resist the greater number of reagents, they naturally remain together, and it is on this account difficult to obtain the one without the other. The fine ether oils obtained from coal-tar by Syme, Thomson, &c., with which they dissolved caoutchouc, and which Thomson called coal naphtha, are therefore never simple, but invariably a combination of mineral-oil and Eupion. A conclusion may be drawn from this investigation of some moment in geology, viz. that coal cannot, as has been in part imagined, be the product or a semicarbonizing heat, and that it can never have been subjected to a very elevated temperature, as, in that case, the mineral-oil must have disappeared, and we should not now find it in our coal. The result of these experiments confirms also the opinion I expressed against Dumas, in my 12th continuation of these essays, viz., that naphthaline, a product of a very high temperature, could not exist in coals, since these have been exposed to no high temperature.

General Results.

1st, The coals of the great coal formation contain about $\frac{1}{320000}$ of an etherial oil, which can be distilled by means of water alone. The coals of the quadersandstone formation (greensand) do not contain it.

2. This oil is physically and chemically identical with petroleum, which therefore

3. Existed previously, already formed, in coals: and therefore,

4. Is no product either of the carbonization or combustion of coal.

5. The artificial mineral-oil presents so many points of analogy with turpentine-oil, both in a physical and chemical point of view, that,

6. Mineral-oil is very probably the turpentine-oil of the pines of an early period in the geological history of the globe.*

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* The resemblance of mineral-oil to the turpentine-oil of pines is a fact illustrative of the important observations of Nicol of Edinburgh in regard to the Coniferæ of the great coal formation of Britain, New Holland, and other ountries.

7. Petroleum wells appear to be feeble distillations of large beds of coal, and produced by the universal subterranean heat of the globe.

8. Coal-beds have never been subjected to a high temperature.

9. Eupion and mineral-oils are quite distinct; rectified coal-tar, however, as it is prepared for dissolving caoutchouc, contains, besides other substances, a mixture of mineral-oil and Eupion.*

ON THE BERLIN CAST-IRON ORNAMENTS.†

The Berlin cast-iron ornaments may be mentioned as an interesting example of the increased value of manufactured, in comparison with the raw material; and we select this manufacture the more willingly, as it had its origin in Prussia; and though many attempts at imitation have been made, has never yet been equalled in any other country. In one of the principal manufactories of this description in Berlin, that of Devaranne, such is the fineness and the delicacy of those separate arabesques, rosettes, medallions, &c. of which the larger ornaments are composed, that nearly ten thousand go to the pound. The price increases in proportion to the fineness, as will be seen by the following table, which gives the selling prices of the above named manufacturer.

	No. to the Cwt.	Price of each Article.			Price per Cwt. of the same.		
		£	s.	d.	£	s.	d.
1. Buckles, 3½ inches long, and 2½ inches broad, ...	2,640	0	2	6	330	0	0
2. Neck-chains, 18 inches long, and 1 inch broad; and composed of 40 separate pieces, ...	2,310	0	6	0	693	0	0
3. Bracelets, 7 inches long, and 2 inches broad; and composed of 72 pieces, ...	2,090 pair.	0	8	6	888	5	0
4. Diadems, 7½ inches high, and 5¼ inches broad, ...	1,100	0	16	6	907	10	0
5. *Sevigné needles*, 2½ inches long, and 1½ inches broad; and composed of 11 parts, ...	9,020	0	4	6	2,02	10	0
6. Sevigné ear-rings, 3 inches long, and ⅞ of an inch broad; and composed of 24 pieces, . .	10,450 pair.	0	5	3	2,743	2	6
7. Shirt-buttons, ...	88,440	0	0	8	2,948	0	0

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* From Schweigger-Seidel's N. Jahrbuch der Chemic und Physik. Heft. 1. B. ix. 133.

† From Dr Friedenberg's German edition of Babbage's work.

If we reckon the price of the grey iron from which these ornaments are made at 6s. per cwt. on an average, we find that the value of the material is increased 1100 *times* in the *coarser* articles, and 9827 *times* in the *finest*.

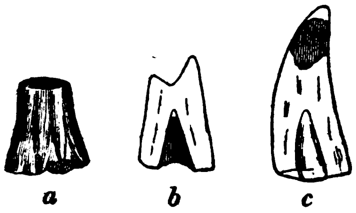
The above are the retail prices, and wholesale prices are probably ⅙th or ⅛th less: but we must remark that, compared with the old prices, the present ones are much fallen. About six years ago they were twice as high, and twelve years ago three times; so that, at that time, Berlin cast-iron was nearly of equal value with gold,—a remarkable example, and perhaps one of the strongest proofs of the influence of the industry of manufacturers on the wealth of the state, especially when we consider that the cast-iron ornaments are made of native material, and exported in large quantities abroad, and even indeed to America. It is so much the more to be regretted, that, owing to the imitation system which already prevails to a great extent, a branch of native industry, once so flourishing, should threaten to fall gradually into decay. The facility of imitation of the most saleable objects, by purchasing them at a low price, using them as models, and then casting articles of the same description, enables the imitator to offer his goods at such a low price, that the industrious original manufacturer who has been at the expense of much time and capital in the designing and forming a brass model, finds it impossible to enter into competition with him. On the one hand, therefore, the manufacturer cannot venture to expend much capital on new models which do not repay the outlay; and on the other, by repeated casting, the articles lose much of their sharpness and beauty, and the natural consequence (and which is already perceptible) is, that their reputation abroad must sink; and notwithstanding the moderate prices, the sale must decline. On this account, some of the best manufacturers have given up the business, and the task of improving and perfecting this branch of industry now rests in the hands of a few.

The piracy of locks is regarded as dishonourable, and against the laws; in technical manufactures new discoveries and improvements can be secured by patents; the cast-iron manufacture only is unprotected, and imitation allowed to be carried on openly and freely.

ADDITIONAL NOTICES RELATIVE TO THE FRESH-WATER LIMESTONES IN THE VICINITY OF EDINBURGH, BELONGING TO THE CARBONIFEROUS GROUP OF ROCKS. *By Dr HIBBERT.*

In a paper lately read before the Royal Society of Edinburgh, Dr Hibbert explained the progress which had been made, since he first pointed out the existence of plants as well as of fish and saurian animals, in the fresh-water limestone of Burdiehouse, towards a farther investigation of these animal remains. The inquiry has been subsequently carried on by the Royal Society of Edinburgh, through their General Secretary Mr Robison.

The more important animal relics, which the limestone quarry of Burdiehouse has yielded, are various kinds of fish, some of them referrible to the extinct genus of the Palaeoniscum, large scales, evidently saurian, exhibiting a most brilliant lustre, and presented in remarkable abundance, the epiphyses of vertebræ, numerous fragments of bones much broken, and teeth which, in their internal structure, give incontestible evidence of the dentition that is peculiar to animals more or less resembling the crocodile, or gavial.*



The above figures represent the section of a tooth, obtained by Mr Robison, which had been accidentally broken in a longitudinal direction.

a shews the root of the tooth.

b is the reverse side of the same, in which a small internal cavity may be observed, indicative of a newer replacing tooth in an incipient state of growth.

c is the larger fragment of the tooth in which the newer tooth, of a conoid form, (protruded from its alveolus) is contained.

The cavities of the new tooth, and of the intermediate space of the old tooth, are at present filled with earthy substance.

The author then proceeded to point out other localities in which beds of fresh-water limestone crop out.

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* A wood-cut representation of the first saurian relic discovered in the quarry by Dr Hibbert, consisting of a large tooth in a beautiful state of integrity, was given in the last number of the Journal. See the Author's letter to Professor Jameson, page 193.

At East Calder, and to the south-west of Mid Calder, the limestone which is there quarried appears, like that of Burdiehouse, to have a fresh-water origin. Its strata have undergone great derangement, and dip in various directions. In one of the quarries of East Calder, where a good section is exposed, the lowest rock is said to be sandstone, above which the following strata may be enumerated in an ascending order:—A yellowish coarse limestone, 16 feet thick;—limestone, 43 feet thick, in which vegetable remains are contained, such as are usually found in coal-fields, and, along with these, scales of Saurian reptiles have been discovered;—nine feet of a very bituminous shale, part of which burns readily, mixed with ironstone;—shale (named Blaes) 16 feet;—and, at the top of the series, an alluvial covering of clay, sand, &c. in which large boulders occur.

Another site, where a fresh-water limestone crops out, is Kirkton, situated a mile or more east of Bathgate. Very interesting phenomena are here exhibited. The chemical action under which the deposit was elaborated, appears to have been so powerful as to have caused such miscellaneous earthy matters as are found to enter into the composition of an impure limestone, like that of Kirkton, to separate into laminæ, and to assume a sort of striped disposition, (*rubané*, as it is also named), resembling what the author has occasionally noticed in Auvergne, where tertiary strata have come into contact with volcanic rocks. The strata, for instance, of Kirkton quarry are composed of distinct and alternating thin laminæ, some of them being of remarkable tenuity, variously consisting either of pure calcareous matter, of translucent silex, resembling common flint, or of a mixed argillaceous substance, which approaches to the character of porcellanite, or of ferruginous, or even of bituminous layers; the surface of the two latter description of laminæ having often a sort of blistered appearance, as if from the effect of heat. Frequently also, in the purer limestone, a globularly concretional structure is observable. The whole of the strata of Kirkton quarry shew a kind of warping or curvature, which is to be traced no less in small detached specimens of the rock than in the contortions or wavings which are exhibited among the strata upon a large scale.*

All these appearances, in connexion with the remarkable circumstance, that greenish-coloured beds of trap-tuff of igneous origin, originally perhaps ejected in the form of a hot tufaceous mud, are interposed among the strata in divers places, one of which has acquired the thickness of nine feet, lead to the irresistible conclusion, that the calcareous beds of Kirkton in their elaboration were in immediate contiguity with some volcanic focus, and that in their original development they must have exhibited the phenomena of hot springs charged with earthy matters, principally calcareous, such as are familiar to the geologist at the present day, in districts where the volcanic agency is still in activity.

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* This limestone is extensively quarried for burning, and the Author has understood, that, although very impure, it possesses qualities which particularly recommend it to the use of the agriculturist. These are well deserving farther investigation.

From this fresh-water limestone the author collected several plants, viz. Ferns, &c. of the same kind as are usually found in the carboniferous group of rocks. No remains of fish, as far as he could learn, have yet been detected in the deposit, nor, considering the circumstances under which the limestone was formed, could they perhaps be reasonably expected; but he is inclined to suspect that relics of some amphibious animal allied to the tortoise have been occasionally discovered.

The upper strata of this deposit are either alternated with, or surmounted by, beds of argillaceous shale, mixed with seams of ironstone. The whole of the strata dip to the west or north by west, and are succeeded, as far as can be learned from the covered state of the ground, by alternating beds of sandstone and shale, which, at the distance of less than half a mile from Kirkton quarry, underlie thick limestone beds containing marine shells and corallines. Lastly, all the beds of this vicinity seem to have been surmounted by masses of feldspar rock, occasionally columnar.

The inference to be drawn from these observations, is, that the fresh-water deposit of Kirkton, like that of Burdiehouse, has an earlier date of origin than the marine limestone of the district, and that important geological changes, probably of a gradual nature, had contributed to depress the lacustrine deposits which had thus been formed, beneath the level of some subsequent invading ocean.

Meteorological Table, extracted from the Register kept at Kinfauns Castle, North Britain. Lat. 56° 53′ 30″. Above the level of the Sea 150 *feet.* By the Right Hon. Lord GRAY.

1833.	Morning, ½ past 9. *Mean height of*		Evening, ½ past 8. *Mean height of*		Mean Temp. by Six's Therm.	Depth of Rain in Garden.	No. of Days	
	Barom.	Therm.	Barom.	Therm.			Rain or Snow.	Fair.
	Inches.	°	Inches.	°	°	Inches.		
January, ...	30.046	32.581	30.042	33.677	32.839	0.50	2	29
February, ..	29.183	39.357	29.209	39.321	39.321	4.50	15	13
March, ...	29.758	40.484	29.789	38.774	39.774	2.00	15	16
April, ...	29.538	46.900	29.555	44.167	45.567	2.50	11	19
May, ...	29.870	57.097	29.883	55.349	56.581	1.40	7	24
June, ...	29.523	59.167	29.528	56.067	57.133	4.50	17	13
July, ...	29.751	63.226	29.783	58.387	59.952	4.30	10	21
August, ...	29.698	58.516	29.701	53.258	55.193	1.90	8	23
September,	29.662	54.767	29.705	51.700	52.900	3.20	12	18
October, ...	29.553	50.839	29.562	46.613	48.323	2.65	11	20
November,	29.501	42.300	29.546	40.933	42.067	2.00	13	17
December,	29.200	40.064	29.203	39.645	39.935	3.60	17	14
Average of the year,	29.607	48.775	29.625	46.491	47.465	33.05	138	227

ANNUAL RESULTS.

MORNING.

BAROMETER.			THERMOMETER.		
Observations.	*Wind.*			*Wind.*	
Highest, 8th Jan.	W.	30.52	28th July,	SW.	72°
Lowest, 2d Feb.	W.	28.35	22d January,	SW.	23°

EVENING.

Highest, 8th Jan.	W.	30.52	15th July,	W.	67°
Lowest, 2d Feb.	W.	28.20	22d January,	SW.	24°

WEATHER.	DAYS.	WIND.	TIMES.
Fair, ...	227	N. and NE.	23
Rain or Snow, ...	138	E. and SE.	109
		S. and SW.	155
	365	W. and NW.	78
			365

Extreme Cold and Heat by Six's Thermometer.

Coldest, 22d January, ...	Wind SW.	20°
Hottest, 29th July, ...	do. SE.	78°

Mean Temperature of the year 1833, . . 47°.4653

Results of Two Rain Guages.

	In. 100.
1. Centre of Kinfauns Garden, about 20 feet above the level of the sea, ...	33.05
2. Square Tower, Kinfauns Castle, 180 feet, . . .	33.65

SCIENTIFIC INTELLIGENCE.

ZOOLOGY.

1. *Low, the Orkney Naturalist.*—The Rev. George Low, the author of the *Fauna Orcadensis*, was born in the parish of Edzel, in August 1746. He lost his father at an early age, and was brought up in the care of an excellent mother. He studied one year at Aberdeen, but obtained the principal part of his scientific and theological education at St Salvador College, in St Andrews. He was there a student in 1766, as appears by one of his pocket-books, in Dr Traill's possession. In the summer of that year he went to the Orkneys, as private tutor in a gentleman's family, and soon began to investigate the natural history of those islands with a zeal and intelligence that attracted the notice and lasting regard of Sir Joseph Banks and Mr Pennant, when they happened to touch at Stromness, on their voyage to Iceland. He was encouraged by them to make a tour through the Orkney and Shetland group, in 1773–4, and a part of 1778. The result of his labours was a complete *Fauna* and *Flora Orcadensis;* and he had also prepared a general history of those islands. The *Fauna* has appeared, but the *Flora* has been lost. Last year, however, Dr Traill recovered a MS. of Mr Low, which contains both the *Fauna and a Flora*, with brief but careful descriptions in Latin, more full than the publication edited by Dr Leach, in having a list of the invertebrate animals which he had observed in Orkney; besides several other papers of this eminent but unfortunate man. The principal of these were "Microscopical Observations, in three parts, by G. L.;" written between 1771 and 1773, while he was a private tutor. This work contains much original matter, and many corrections of the descriptions of *Baker* and *Joblet*, illustrated by beautiful china-ink drawings by the author's hand. Dr Traill also has recovered the original notes of Mr Low's Shetland Tour; and the draught of a letter to Pennant, containing a list of the *Fauna* and *Flora Orcadensis.* Mr Low obtained a church living in Orkney, in 1774. In 1775 he married Miss Tyrie, the daughter of the clergyman of the parish in which he had resided. But he had the misfortune to lose

ber in a few months,—a blow from which he severely suffered, but sought consolation in the duties of his ministry, and the study of the works of the Almighty. His intense application to microscopical observations had impaired his sight in 1790; and, from that time till his death in 1795, he suffered from severe headach and irritable ophthalmia, which rendered him almost totally blind for the last two years of his life. On his death his papers and MSS. fell chiefly into the hands of the late Mr George Paton of Edinburgh, on whose death they were dispersed.

2. *Wood's new work on the Mammalia.*—The first monthly part of a new work on Natural History, by Henry Woods, F. Z. S., A. L. S., which has been nearly seven years in preparation, is announced to appear on the 31st of March. It will combine scientific arrangement with copious detail, and form a complete concentration of all that is at present known of the entire class *Mammalia*, embracing the latest discoveries, and including an accurate account of the physiology, habits, locality, &c. of every recognised existing species, including the fossils. The illustrations, which are chiefly portraits of the animals themselves, drawn from nature, expressly for the work, by Harvey, will exceed 500 in number, besides a great variety of osteological and fossil figures, from the drawings of the author; and the entire work will be completed in thirty Monthly Parts.

3. *On Migratory Habits of certain species of Hirundo and Sylvia.*—At a meeting of the Linnean Society, there was read a letter addressed to the Secretary from Charles Lucien Buonaparte, Prince of Musignano, dated on board the Delaware, near Gibraltar, March 20. 1828, containing some curious facts relative to the migratory habits of certain species of Hirundo and Sylvia. The following are extracts:—In closing my letter I happen to think that the following fact may be thought interesting to some of your ornithological gentlemen. A few days ago, being 200 miles from the coasts of Portugal, 400 from those of Africa, &c., we were agreeably surprised by the appearance of a few swallows (*Hirundo urbica* and *rustica*). This, however extraordinary, might have been explained by an easterly gale, which might have cut off the swallows migrating from the Main to Madeira, only 200 miles distant from us; but

what was my surprise, in observing several small warblers hopping about the deck and riggings. These poor little strangers, exhausted as they were, were soon caught and brought to me. The following is a list of the species:—1. *Sylvia Trochilus*; 2. *Sylvia erithacus*, Lath. (*Tithys*, Temm.); 3. *Sylvia suecica*, or rather a similar species, which I have already received from Egypt and Barbary; 4. A species new to Europe, and perhaps even a nondescript, having the plumage of an *anthus*, and which, I think, belongs (as *Sylvia cisticola* and others) to the hitherto African genus *Malurus*. This, however, must rest undecided, my specimen having lost its tail, which had been pulled off by the sailor who caught the bird."—*Extract from the Min. Book of Linn. Soc.* vol. xvi. part iii. p. 754.

4. *On the Habits of some Land Shells, by John Curtis, Esq. F. L. S.*——" *Grove Place, 2d May* 1831.—Dear Sir, On my return from France, I brought home some land shells, which I collected near the celebrated fountain of Petrarch at Vaucluse, on the 8th of last July, at which time they were close packed in a pill-box; and from the high temperature of that part of France, and being kept for several weeks in my trunk, and afterwards in a dry place at home, they appeared, as might be expected, quite dead. I was induced, however, a few days since, to try if they could be reanimated, although I almost thought it an useless experiment. I put the shells into an earthen vessel close covered, and containing some wet moss, when, to my astonishment, in less than twenty-four hours, these little animals were reanimated and crawling about, after having been shut up without food or moisture for nine months. The shells appear to be the *Pupa tridens* and the *Clausilia rugosa*, which renders it more remarkable, since they are species destitute of opercula. I observed that only one of the shells was adhering to another, and the others were quite loose in the box.—It is not only the extraordinary fact of these little animals being able to remain so long in a torpid state, that has induced me to request that you will do me the favour to lay these observations before the Linnean Society; but I think it may be of service to those who collect shells, to know that the species inhabiting the land may be preserved for so long a period; for it may in many instances enable those conchologists who wish to describe and draw the

inhabitants of shells, to accomplish that desirable object; and probably, by securing them in a well-stopped bottle, they might be kept alive much longer, and be transported from very remote parts of the globe.—I remain, &c. JOHN CURTIS.

"P. S. I have been informed by Mr Lyell, that some shells brought from South America by Lieutenant T. Graves, were seventeen months without food, and are now alive, and inhabiting their native plants in the Conservatories of Messrs Loddiges at Hackney. But shells closed by an operculum, have been known to remain thus hermetically sealed in cabinets for very long periods,—it has been said for forty years, and afterwards been reanimated by moisture." Some live specimens of the species referred to in the letter, were exhibited at the meeting.—*Extracts from the Min. Book of the Linn. Soc.* vol. xvi. part 3, p. 744.

5. *Shower of Fishes.—June* 15, Read an extract of a letter from Mrs Smith, dated Monradabad, July 20. 1829, to a gentleman in Somersetshire, giving an account of a quantity of fishes that fell in a shower of rain at that place. Many were observed by Mrs Smith from the window of her residence, springing about on the grass immediately after the storm. The letter was accompanied by a drawing taken on the spot, which represents a small species of *Cyprinus*, 2¼ inches in length, green above, silvery white below, with a broad lateral line, bright red.—*Ext. from Min. Book of Linn. Soc.* vol. xvi. part 3, p. 764.

NEW PUBLICATIONS.

1. ***Illustrations of the Botany and other Branches of the Natural History of the Himalayan Mountains, and of the Flora of Cashmere.*** **By J. FORBES ROYLE, Esq. F. L. S. & G. S., &c. of the Honourable East India Company's Medical Establishment, &c. Part I., with 22 Coloured Engravings. Folio. Parbury, Allen, & Co. London, 1833.**

We formerly announced the promised appearance of this important work. A perusal of the very interesting letter-press, and a careful examination of the well engraved and beautifully coloured plates of Himalayan plants and animals, in this the first number, fully realise the very favourable opinion we expressed of Mr Royle's Illustrations, an opinion founded on the well known and highly esteemed practical skill of our author as a naturalist, and his activity and intelligence as a traveller. The forty pages on the geographical distribution of the plants and the animals of the Himalays, will be read with pleasure and delight, even by those not very deeply versed in the minutiæ of Natural History. The getting-up of the work is highly creditable to the publishers.

2. *An Outline of the Geology of Norfolk.* By Samuel Woodward, Member of the Yorkshire Philosophical Society, and Author of a Synoptical Table of British Organic Remains. 8vo, with a Coloured Map and Sections, and Six Plates of Organic Remains. Norwich, 1833.

We recommend this interesting work to the attention of geologists, and the examination of the district described by Mr Woodward, to those studying the newer Neptunian deposits, as the country around Norwich, affords Oolite, Carstone, Chalk Marl, Hard Chalk, Medial Chalk, Upper Chalk, Crag, Blue Clay, and Alluvium.

3. *Entomologia Edinensis; or, a Description and History of the Insects found in the neighbourhood of Edinburgh.* By James Wilson, F. R. S. E., M. W. S., &c.; and the Rev. James Duncan, M. W. S. W. Blackwood, Edinburgh, and T. Cadell, London. 1834.

It affords us great pleasure to call the attention of our readers to the work above named. The volume now published contains the order *Coleoptera*, a department selected by the authors as the most complex and extensive, as well as that which has of late years been the most sedulously studied, and therefore likely to prove of the highest interest to the science of entomology. We have long regretted that a field so interesting as that presented by the vicinity of Edinburgh, should have remained till recently in a great measure almost unexplored; for no doubt can be entertained of its being well adapted, by its physical and local characters, to reward the labours of the collector.

The diversified nature of its soil and surface, and even the range of temperature and atmospheric pressure arising from the inequalities of elevation, afford appropriate localities to species of very different habits, and thus present, as it were, a kind of *microcosmic* view of British Entomology. The indented shores of our beautiful and far-flowing Frith, offer an extended field for the occurrence of littoral species, and such as affect a sandy soil; while the Pentland Hills, attaining, even in their nearer range, to any elevation exceeding 1700 feet, produce examples of the kinds more characteristic of heathy grounds or upland pastures. The intermediate and undulating plains are rich in gardens, and other grounds of varied culture, and are intersected by occasional streams, the banks of which, sloping or precipitous, exhibit a diversified vegetation, favourable to the occurrence of numerous forms of insect life. Although the district is not richly wooded, yet there are many belts of thriving plantations, and even occasional groups of "old ancestral trees," among whose leafy umbrage, we doubt not, many of the woodland tribes still remain to be discovered.

The only previous contributions to Scottish Entomology with which we are acquainted, are Mr Stewart's "List of Insects found in the neighbourhood of Edinburgh," which contain scarcely more than a hundred species of coleoptera; and Mr Duncan's "Catalogue of Coleopterous Insects," from the same vicinity, in which the amount was at once extended to nearly 550. These papers were published in the *Wernerian Memoirs*, vol. i. p. 566, and vol. vi. p. 443. The joint-volume now published by Messrs Wilson and Duncan, contains, we observe, between 600 and 700 species; and we doubt not, from the impulse which a work executed with such accuracy and intelligence is likely to give to the subject, that great accessions will henceforward be made from year to year. Besides the description and history of the species captured in the Edinburgh district, the work contains the names and localities of all the coleopterous insects hitherto known to have been seen in Scotland; so that, while its principal portion, relating to a limited sphere, will be chiefly useful within those limits, its incidental notices of numerous other species, will render it interesting even to those at a distance, especially to such as feel the value of every contribution to our knowledge of zoological geography. In truth, the great amount of species actually described (comprised in upwards of 200 genera) has necessarily introduced the general history of the leading groups in British Entomology; and as all of these occur in other parts of the country, the utility of the work is by no means confined to the district specified, but will extend to any portion of the empire.

The present volume contains an introduction, in which the principal features of the anatomy, physiology, and general characteristics of the class of insects, are sketched in a brief but luminous and pleasing style; and it is terminated by an addendum containing above sixty species of coleoptera discovered in Scotland by the authors or their friends, while the sheets were passing through the press. We observe that they have omitted to mention the occurrence of the rose-beetle, *Cetonia aurata*, which we know has been found in the shires of Moray and Dumfries.

PROCEEDINGS OF THE SOCIETY FOR THE ENCOURAGEMENT OF THE USEFUL ARTS IN SCOTLAND.

The following election took place on 11th December 1833:—

The KING, Patron.

OFFICE-BEARERS FOR SESSION 1833–34.

President, . . . His Grace The DUKE OF BUCCLEUCH and QUEENSBERRY.
Vice-Presidents, { Sir David Milne, K. C. B., F. R. S. E.
John Robison, Esq. Sec. R. S. E.
Secretary, . . . James Tod, Esq. W. S. 21, Dublin Street.
Foreign Secretary, William Crawfurd, Esq. 5, Bellevue Crescent.
Treasurer, . . . Robert Allan, Esq. F. R. S. E. Banker, Royal Exchange.
Curator, ... Mr John Dunn, Optician, 50, Hanover Street.

ORDINARY COUNCILLORS.

Jo. Clerk Maxwell.
Rev. Dr Gordon.
Lt.-Col. W. Macdonald.
Archibald Horne.
Ro. Stevenson.
Professor Forbes.
Robert Stein.
Rev. Ed. Craig.
Sir T. D. Lauder.
James Jardine.
Geo. Buchanan.
Jo. G. Kinnear.

The following communications were laid before the Society during the months of January and February 1833:—

January 8.—1. Description and drawings of an Improved Ventilating Warm Air Stove. By Mr Andrew Symmington, Kettle, Fife.

2. On the Mode of Constructing Public Buildings in relation to the Theory of Sound, so as the Voice of the Speaker may be distinctly heard; with illustrative Diagrams. By Mr William Reid, Architect, 27. Charlotte Street, Glasgow.

3. Model and Description of a Self-acting Apparatus for supplying Water to the Boilers of High Pressure Steam-Engines. By Mr Henry Spears, Auchtertool, Fife.

4. Notice regarding Mr Ballingall's Plan for improving the Mercantile Navy of Great Britain and Ireland; and on the present mode of Classifying Merchant Ships. Communicated by Mr James Ballingall, Kirkaldy.

The following Candidates were admitted Ordinary Members, viz.

Mr Robert Ritchie, Ironmonger to the King, 241. High Street, and 80. George Street, residing at 54. India Street, Edinburgh.
Mr James Smith, 8. Carlton Place, Edinburgh.
Mr John Smith, 8. Carlton Place, Edinburgh.

January 22.—1. Description and Drawing of an Improved Cannon. By Mr Andrew Symmington, Kettle, Fife.

2. Model and Description of a Window calculated to secure the safety of Glaziers and Painters. By Mr Thos. Rutherford, Sievewright, Haddington.

3. Description of an Accurate and Cheap Air-Pump, constructed at the suggestion of Charles Chalmers, Esq. By Mr John Dunn, optician, 50. Hanover Street, Edinburgh, Cur. Soc. Arts.—The instrument was exhibited, and some experiments performed with it.

4. Donation.—Additional Specimens of Lithography. By Mr Samuel Leith, lithographer, Banff, Assoc. Soc. Arts.

5. Donation.—Specimen of the New Edition of the Encyclopædia of Gardening. By J. C. Loudon, Esq. Hon. Memb. Soc. Arts. —From the Author.

The following Candidates were admitted Ordinary Members, viz:—

William Murray, Esq. of Henderland, 126. George Street, Edinburgh,
James Wier, Esq. M. D. Lynedoch Place, Edinburgh.

February 5.—1. An Essay on the Causes of Obstructions in Water Pipes and Syphons by disengaged Air; and the Construction of an Air Extractor for removing them. By J. S. Hepburn, Esq of Colquhalzie, by Crieff.—A working Model and Drawings of the Air Extractor was exhibited.—Directions for experimenting with the Model were read.—Supplement to the Essay, being description and drawing of an Apparatus for giving a self-acting power to the Air Extractor.

Feb. 19.—1. An Essay on certain Improvements on the Syphon—for Draining Marshes, Mines, and Lakes; taking a much more effectual advantage of Waterfalls—in raising water for the supplying of Towns, and driving Machinery—than hitherto done. By Mr Jonathan Davidson, ironmonger, 123. High Street, Edinburgh.—Models and Drawings were exhibited.

2. Model and Description of an Improved Chimney Can, for the more effectual curing of Smoky Vents, and preventing the return of smoke occasioned by downward currents, or other causes. By Mr James Shillinglaw, builder, 5. Cheyne Street, Stockbridge.

John Hamilton Colt, Esq. of Gartsherie, 14. Regent Terrace, Edinburgh, was admitted an Ordinary Member.

LIST OF PATENTS GRANTED AT EDINBURGH FROM 23D SEPTEMBER 1833 TO 19TH MARCH 1834.

1833.

Sept. 23. To John Robertson of Crofthead, in the parish of Neilston and county of Renfrew, cotton-spinner, for an invention of "certain improvements in the mule, jenny, or other machine for spinning of cotton, and in the billy, stretching-frame, or other machine for roving of cotton, and in the machinery for spinning and roving of silk, wool, flax, hemp, or other fibrous substances."

Oct. 9. To John Paterson Reid, of the city of Glasgow, merchant and power-loom manufacturer, and Thomas Johnson, of the said city of Glasgow, mechanic, in the employment of John and Archibald Reid, of the said city of Glasgow, power-loom manufacturers, for an invention of "certain improvements applicable to certain looms for weaving different sorts of cloth."

10. To Henry Ewbank, of Idol Lane, in the city of London, merchant, for an invention of "an improved process to be used in dressing, of paddy or rough rice," invented by him in conjunction with his late partner Jonathan Lucas, deceased.

14. To William Wilkinson Taylor, of Bow, in the county of Middlesex, felt-manufacturer, for an invention of "an improved cloth for the sails of ships and other vessels."

21. To William Henry Barnard, of 26. New Broad Street, in the city of London, gentleman, for an invention of "a solvent not hitherto used in the arts."

To James Jones, of Salford, in the county of Lancaster, machine-maker, for an invention of "certain improvements in the making of rovings, spinning and doubling cotton, silk, flax, and other fibrous substances."

22. To John Ericsson, of Albany Street, Regent's Park, in the county of Middlesex, civil engineer, for an invention of "an engine for producing motive power, whereby a greater quantity of power is obtained from a given quantity of fuel than heretofore."

Nov. 7. To Charles Terry, of Shoe Lane, in the city of London, merchant, and William Parker, of New Gravel Lane, Shadwell, in the county of Middlesex, merchant, for an invention of "improvements in making and in refining sugar."

25. To Charles Attwood, of Whickham, near Gateshead, in the county of Durham, glass-manufacturer, for an invention of "a certain improvement or improvements in manufacturing or purifying soda."

To Herman Hendricks of Dunkirk, in the kingdom of France, but now of the Strand, in the county of Middlesex, gentleman, in consequence of a communication made to him by a certain foreigner resident abroad, for an invention of "improvements in manufacturing prussiate of potash and the prussiate of soda, and improvements in dyeing blue colours without indigo."

To Charles Joseph Hullmandell, of Great Marlborough Street, in the county of Middlesex, printer, for an invention of "a certain improvement in the art of block-printing, as applied to calico and some other fabrics."

To George Frederick Muntz of Birmingham, in the county of Warwick, roller of metals, for an invention of "an improved manufacture of boilers used for the purpose of generating steam."

Dec. 10. To John Tennant, merchant, and Thomas Clark, chemist, both of Glasgow, in the county of Lanark, for an invention of "new or improved apparatus to produce or evolve chlorine for manufacturing purposes."

23. To John Babtist Constantin Torassa, of Newington Causeway, in the county of Surrey, gentleman, Paul Isaac Muston, of Austin Friars, in the city of London, merchant, and Henry Walker Wood, of the same place, merchants, in consequence of a communication made to them from Mr Emanuel Montebruno of Genoa, for an invention of "certain improvements in making or producing the pigment commonly known by the name of white lead, or carbonate of lead."

To Bartholomew Richard Comte de Predaval, of Leicester Place, Leicester Square, in the county of Middlesex, engineer, for an invention of "an engine for producing motive power applicable to various purposes."

To William Godfrey Kneller, of Mitcham, in the county of Surrey, chemist, for an invention of "certain improvements in evaporation."

27. To Mark Cosnaham, of the Isle of Man, Esquire, for an invention of "certain improvements in apparatus, modes, or processes, for converting sea or salt water, (and also other brackish, turbid, or impure waters,) into purified or fresh water, which apparatus, modes, or processes, or parts thereof, may be applied to other purposes."

1834.

Jan. 10. To Miles Berry of 66. Chancery Lane, in the parish of St. Andrews, Holborn, in the county of Middlesex, mechanical draftsman, in consequence of a communication from a foreigner residing abroad, for an invention of "an improved apparatus for boiling, evaporating, and concentrating syrups for the production of sugar, and also of saline liquors, or for the crystallization of salt, which apparatus may also be employed in the process of distillation."

To John Joyce, of Sidmouth Street, Gray's Inn Road, in the parish of St. Pancras, in the county of Middlesex, gentleman, in consequence of a communication to him by a certain foreigner residing abroad, for an invention of "a certain improvement, or certain improvements, in machinery for making nails of iron, copper, and other metals."

15. To David Rowland, of No. 68. Crawford Street, in the parish of St

Mary-le-bone, in the county of Middlesex, mechanic, for an invention of “an improvement in the manufacture of sextants, quadrants, circles, and other instruments used in taking observations and surveys.”

1834.

Jan. 23. To John Squire of Paddington Bason, engineer, and Francis Macerone of Upper George Street, Bryanstone Square, Esq. both in the county of Middlesex, for an invention of “certain improvements in boilers for generating steam.”

To Robert Beart of Godmanchester, in the county of Huntingdon, miller, for an invention of “certain improvements in making or producing tiles for draining land, building, and other purposes.”

Feb. 3. To William Rodger of Norfolk Street, Strand, in the county of Middlesex, Lieutenant, R. N., for an invention of “a certain improvement or improvements in anchors.”

10. To Henry Davey of the parish of St Giles, Camberwell, in the county of Surrey, gentleman, in consequence of a communication made to him by a certain foreigner, residing abroad, for an invention of “certain improvements in machinery, or apparatus for preparing linen, and cotton-rags, and other materials used in the manufacture of paper.”

Mar. 11. To Ernst Wolff late of Leeds, in the county of York, but now of Stamford Hill, in the county of Middlesex, gentleman, in consequence of a communication to him by a foreigner residing abroad, for an invention of “certain improved means of supplying heated air, in order to support combustion in enclosed fire places.”

12. To Thomas Wetch of Manchester, in the county of Lancaster, for an invention of “a new method of taking up for power and hand looms.”

19. To James Smith of Deanston, in the parish of Kilmadoch, in the county of Perth, cotton-spinner, for an invention of “certain improvements in machinery, used in the preparing and spinning of cotton, flax, wool, and other fibrous substances.”

To James Smith of Deanston, in the parish of Kilmadoch, in the county of Perth, cotton-spinner, for an invention of “certain improvements in machinery for carding cotton, flax, wool, silk, and other fibrous materials.”

INDEX.

Arnot, G. A., Esq. characters of three new genera of Indian plants, 314
Address delivered by Professor Whewell in the Senate-House at Cambridge, on June 25. 1833, 88
Africa, Central, projected expedition into, 107
Anatomy of the rorqual whale, 181
Animals depicted on antique monuments, account of, 168, 285
Arago, his historical eloge of Alexander Volta, 1—on the thermometrical state of the terrestrial globe, 205
Arts, Society of, for Scotland, proceedings of, 197, 396
Atmosphere, colour of, observations on, by Count Xavier de Maistre, 56
Aurora borealis, instructions for observers of their height, sound, periods, effect on the magnetic needle, 35

Berlin cast-iron ornaments, account of, 384
Blood, experiments and observations on its arterialization, by Dr William Gregory, 285
Bone caves in the south of France, observations on, 302
Burdiehouse fresh-water limestone, observations on, by Dr Hibbert, 190—additional notices relative to the fresh-water limestones in the vicinity of Edinburgh, belonging to the carboniferous group of rocks, by Dr Hibbert, 386

Cascarilla, the bark of, observations on, by David Don, Esq. 367
Coldstream, Dr, his account of the Limnoria terebrans, a minute crab, which proves very destructive to wooden erections, as piers, 316
Chronometer signal, account of its erection at Greenwich, 158
Coniferæ, observations on, by William Nicol, Esq. 137—additional observations on recent and fossil coniferæ, by William Nicol, Esq. 310

Curtis, John, Esq. on the habits of some land-shells, 392
Cuvier as a naturalist, by C. L. Laurillard, 340—his eloge of Arago, 1

Davy, Dr John, his experiments and observations on the combinations of carbonic acid and ammonia, 255
Don, David, Esq. some remarks on the plant which yields the cascarilla bark, 367
Duncan, Rev. James, his Entomologia Edinensis noticed, 394

Electro-magnetic experiments, account of, 71
Electricity and magnetism, observations on, by Professor M. A. de la Rive, 266
Entomologia Edinensis, Messrs Wilson and Duncan's new work on, noticed, 394
Extinct animals, observations on, by Marcel de Serres, 168, 265

Fishes, shower of, 393
Fossil tooth, in red sandstone, found by Lord Greenock, and described by Dr Grant, 38

Galbraith, Mr, his observations on trigonometrical surveying, 110
Gairdner, Dr M., his observations during a voyage from England to Fort Vancouver, on the north-west coast of America, 290
Geology of the Valley of Oodipoor, by Dr Hardie, 59, 278
Geology of Norfolk, Outlines of, noticed, 394
Graham, Dr, his account of new and rare plants, 175
Granada, observations on the tertiary formation of, by Brigadier Silvertop, 45
Grant, Dr R., his account of a fossil-tooth found in red sandstone, 38
Gray, Right Hon. Lord, meteorological table kept by him at Kinfauns Castle, 389
Gypsies, remarks on, 67

Hardie, Dr James, his geology of the valley of Oodipoor, 59, 278
Heat, development of, in flowers of Caladium pinnatifidum, by Dr Schultz of Berlin, 88
Hibbert, Dr, his observations on the Burdiehouse limestone and its saurian remains, 199—additional notices, 386

Iron slags, on the composition of, by J. F. W. Johnston, 190

Land-shells, on the habits of some of them, 392
Low, the Orkney naturalist, 390

Macgillivray, William, his remarks on Mr Nicol's observations on the structure of recent and fossil Coniferæ, 369
Man, weight of, at different ages, observations on, by Quetelet, 334
Marcel de Serres on extinct animals, 168, 285
Medicine, state of, in Turkey, by Dr Oppenheim, 114
Memoir on the question, Whether any land animals have ceased to exist since man's formation? and, Whether man has been contemporaneous with species now lost, or appearing no longer to have representatives on the earth? by Marcel de Serres, 168, 285
Meteorological Table kept at Kinfauns Castle, 389
Meteoric stones, origin of, by F. G. Fischer, Esq., 75
Method of so far increasing the divergency of the two rays in calcareous spar that only one image may be seen at a time, 372
Migratory habits of certain species of Hirundo and Sylvia, 391

Nicol, William, observations on recent and fossil Coniferæ, 187—additional observations on recent and fossil coniferæ, 310

Oodipoor, its geology, by Dr Hardie, 59
Oppenheim, Dr, his observations on the state of medicine in Turkey, 114

Patents, list of, granted at Edinburgh from 24th June to 27th August 1833, 204, 398
Proofs that the human bones and works of art found in caves in the south of France, are more recent than the antediluvian bones found in these caves, 302
Petroleum or mineral-oil, observations and experiments on, by Dr Reichenbach, 376

Quetelet, his inquiries respecting the weight of man at different ages, 334

Rattlesnake disarmed by the leaves of the white ash (Fraxinus americana, Mich.), by Judge Woodruff, 43
Rive, Professor De la, his observations on electricity, and on the natural sources of electricity and magnetism, 266

Reichenbach, Dr, his observations and experiments on petroleum or mineral-oil, 376
Royal Society of Edinburgh, proceedings of, 181
Royle, Mr J. F., notice of his Illustrations of Botany, and other branches of the natural history of the Himalayan mountains, 393

Silvertop, Brigadier, his sketch of the tertiary formation in the province of Granada, 45
Societies, proceedings of,—of Arts for Scotland, 197, 396—Royal Society of Edinburgh, 181—Wernerian Natural History Society, 195
Statistic views of the mortality in various counties in Europe, 259
Statutes of the University of Edinburgh relative to the degree of M. D. 1833, 200

Trigonometrical surveying, observations on, by Mr Galbraith, 110

Volta, Alexander, his eloge, by Arago, 1

Wauchope, Capt. R., his account of the establishment of his signal for the ascertaining the rates of chronometers at the Royal Observatory, Greenwich, 158
Weight of man at different ages, inquiry about, 334
Wernerian Natural History Society, proceedings of, 195
Whewell, Rev. W., his address, delivered in the Senate-House at Cambridge, June 25. 1833, 90
Wilson, James, Esq. his Entomologia Edinensis noticed, 394
Wood, Mr Henry, his new work on the Mammalia noticed 391
Woodward, Mr Samuel his Outline of the Geology of Norfolk noticed, 394

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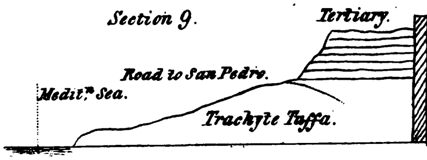
PLATE I.

Section 9.



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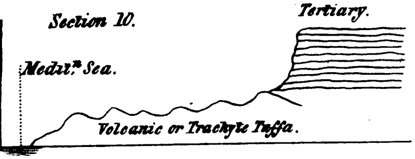

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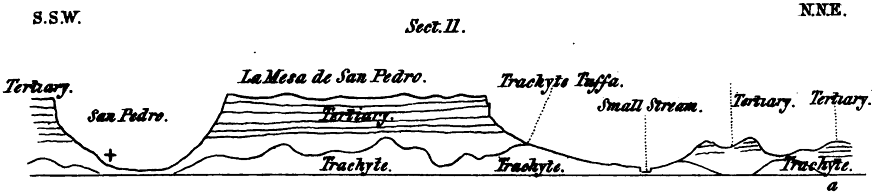

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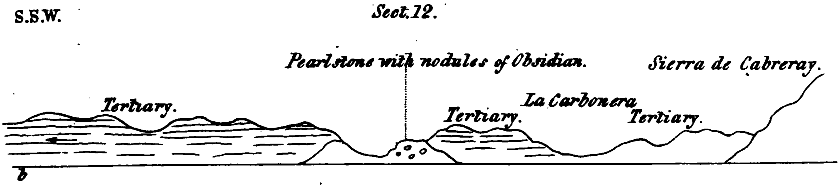

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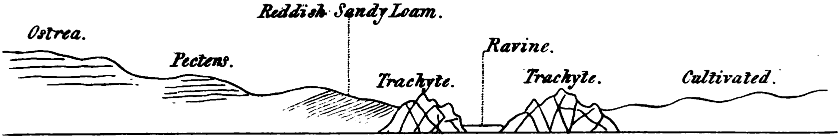

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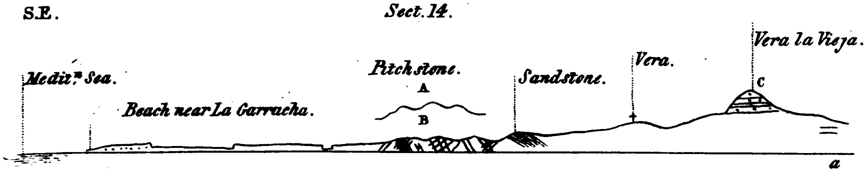

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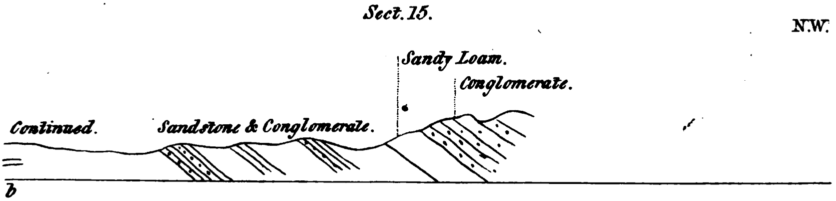

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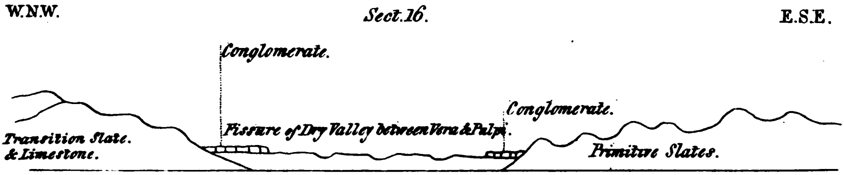

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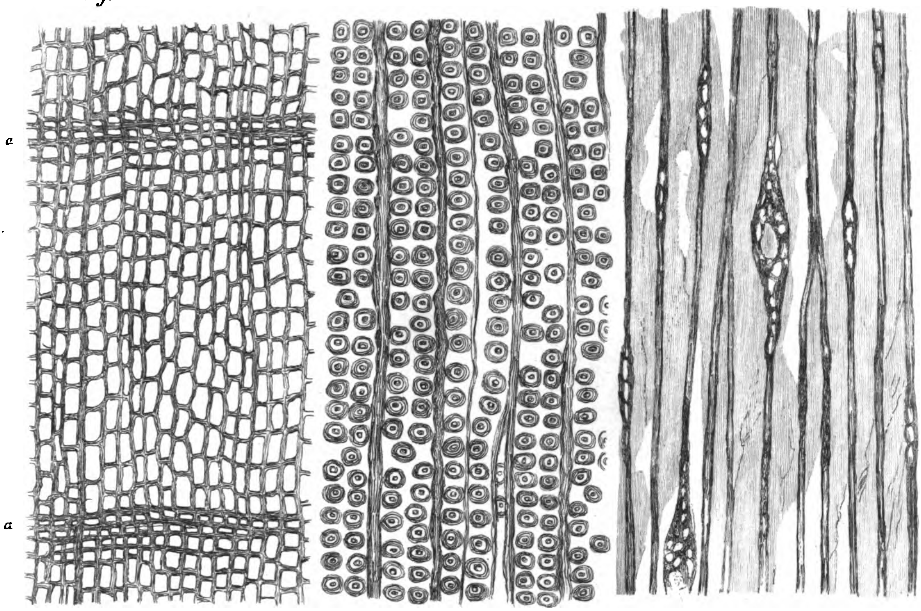
PLATE II. Edin^r. new Phil. Jour. Vol XVI. p. 156.

Fig. 1. 2 3



Section's of Pinus Strobus.

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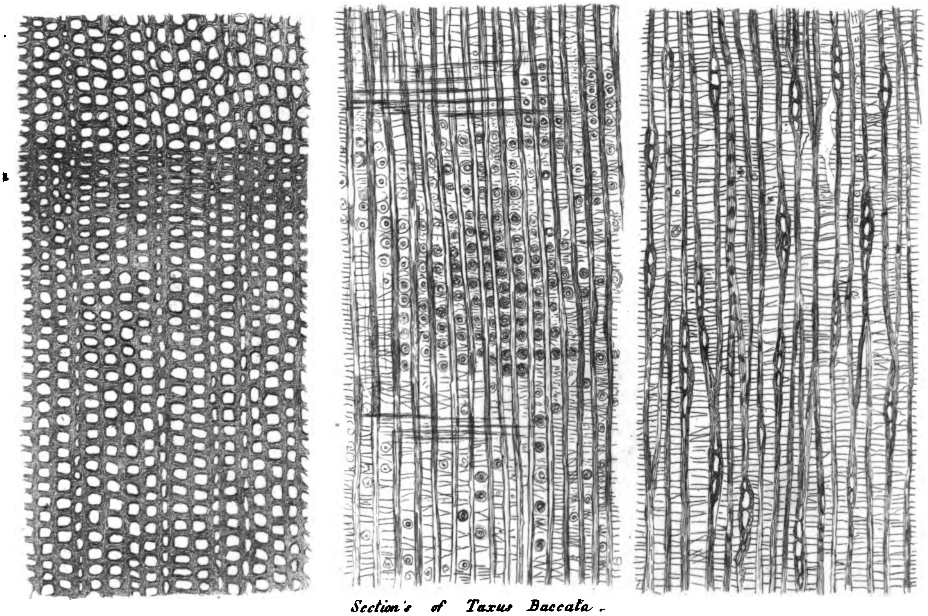

Section's of Taxus Baccata.

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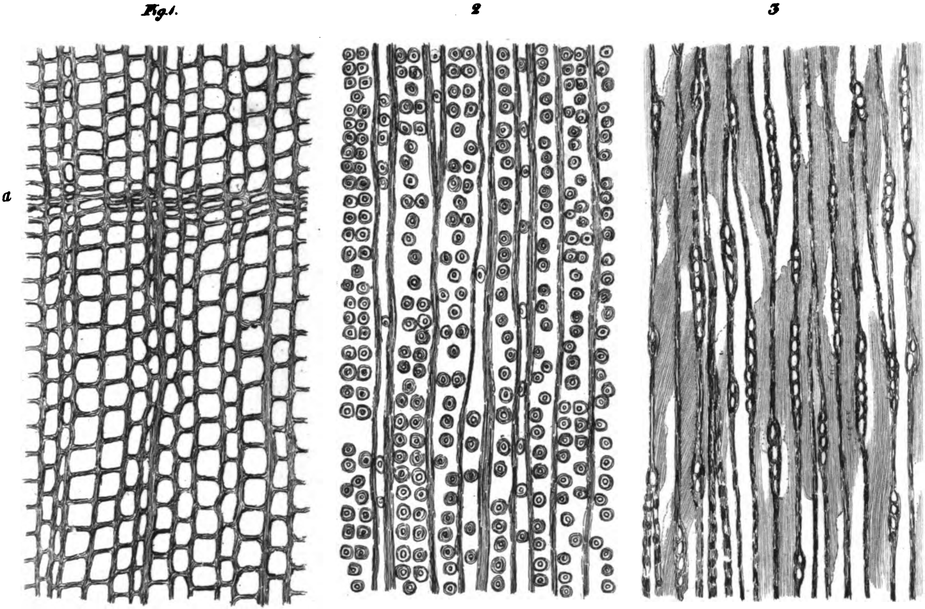
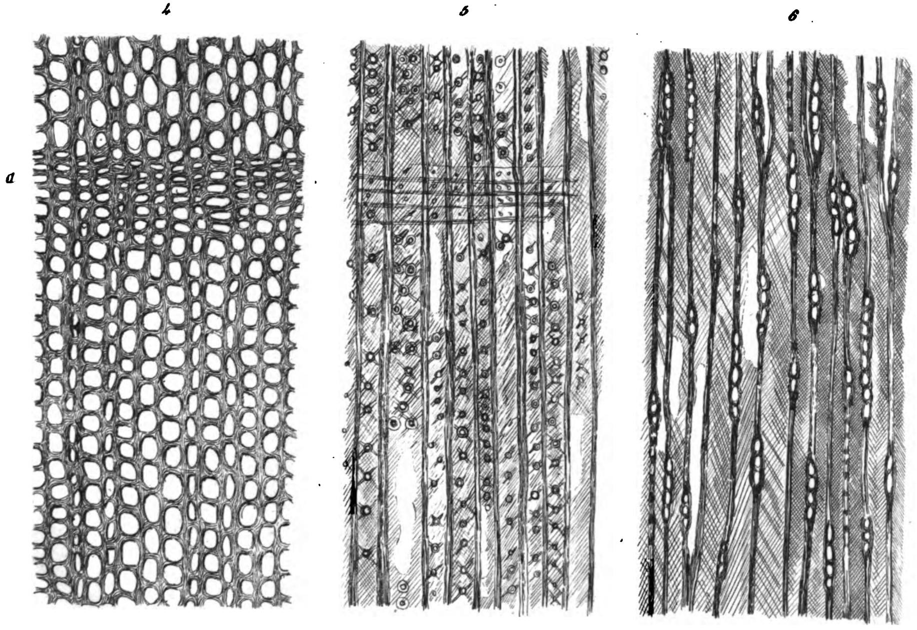
PLATE III.

Edin. new Phil. Jour. Vol. XVI. p. 158.



Sections of Taxodium Disticha.



Sections of Taxodium Disticha.

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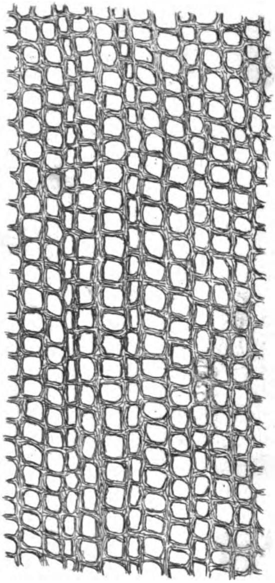
PLATE IV. *Edinr. new Phil. Jour. Vol. XVI. p. 158.*

Fig. 1.



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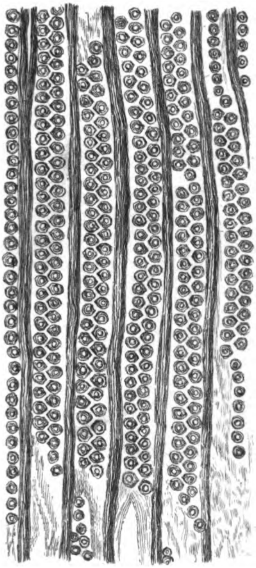

Sections of Araucaria.

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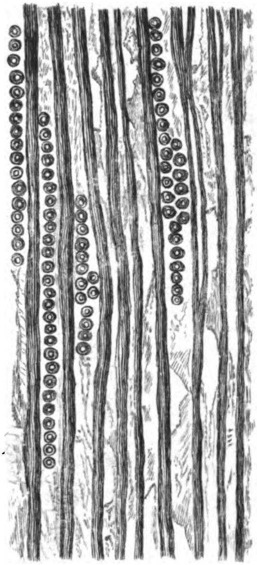

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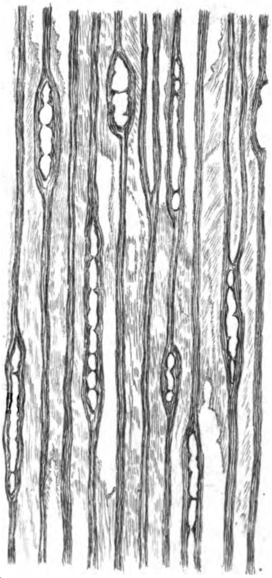

Sections of Araucaria.

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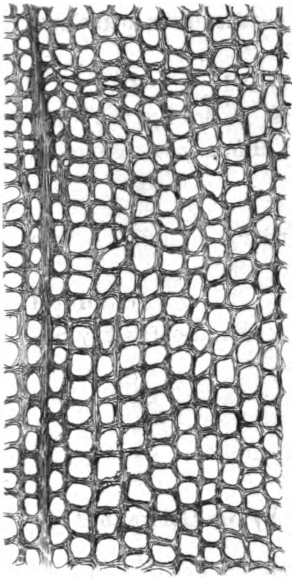
PLATE. V. Edin^{r} new Phil. Jour. Vol. XVII. p. 314.

Fig 1



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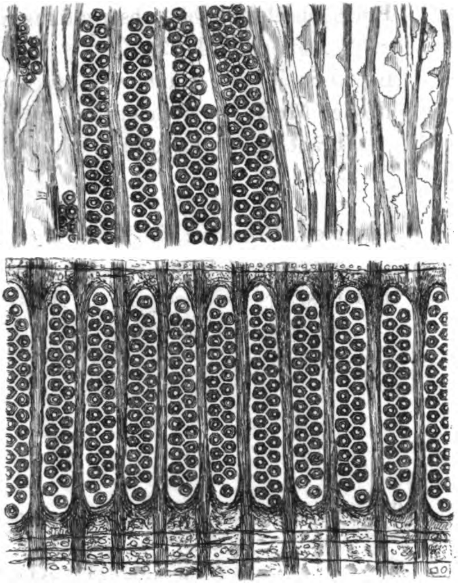

Araucaria Excelsa.

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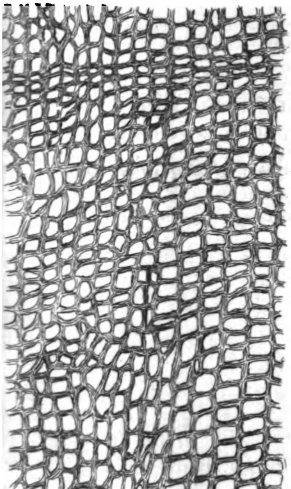

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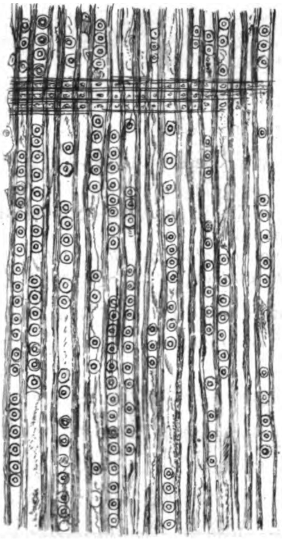

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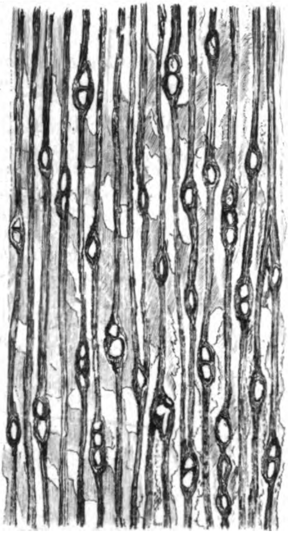
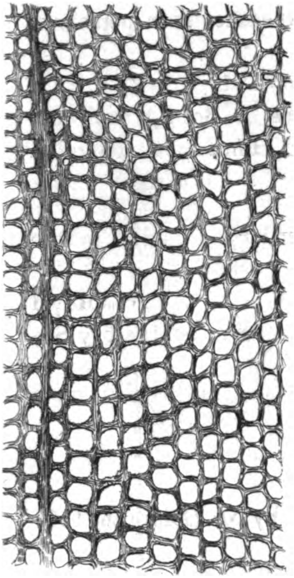


W. Scott Del^{t} *Salisburia Adiantifolia.* E. Mitchell Sculp^{t}

PLATE. V.   Edin.r new Phil. Jour. Vol. XVII. p.314.

Fig 1



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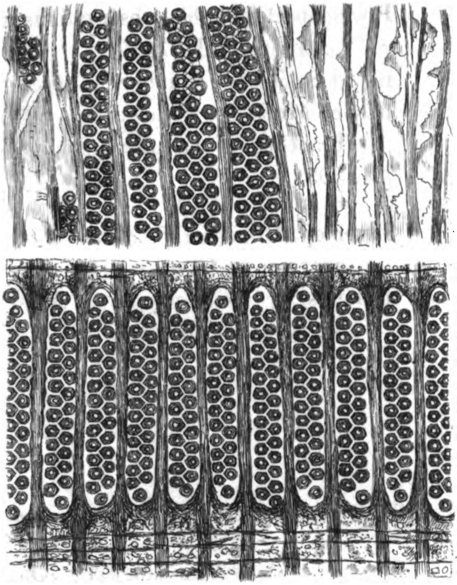

Araucaria Excelsa.

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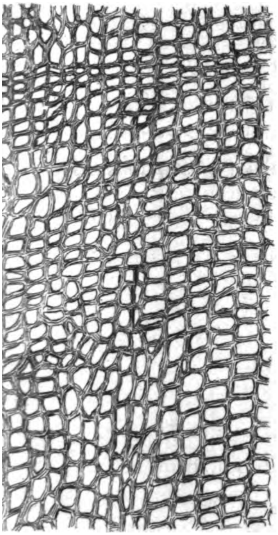

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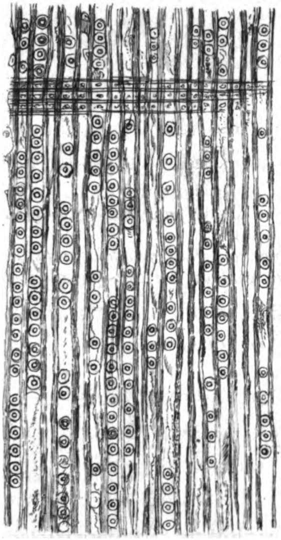

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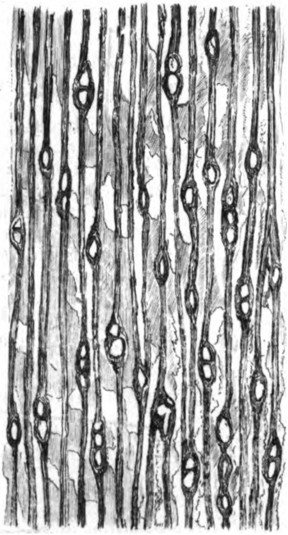

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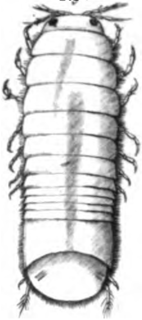
Salisburia Adiantifolia.

E. Mitchell Sculp.t

PLATE VI.

Edinʳ new Phil. Jour. Vol. XVII. p. 333.

Fig. 1



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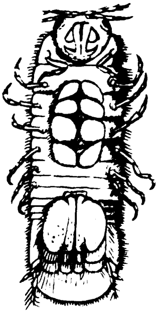

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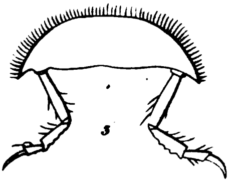

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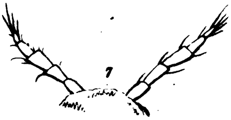

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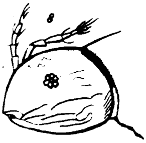

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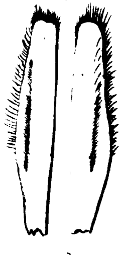

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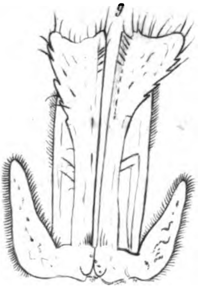

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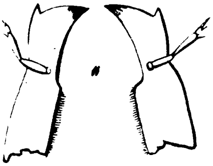

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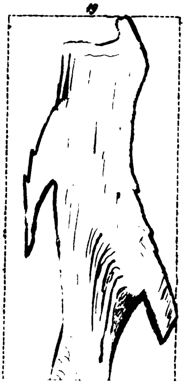

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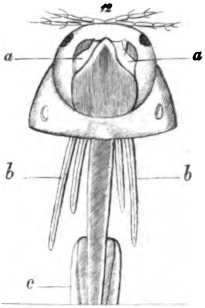

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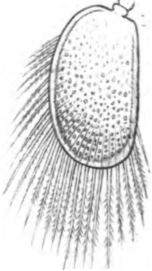

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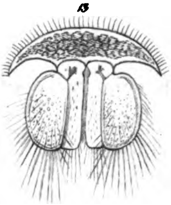

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